

**REPRESENTATION MODELS AS DEVICES
FOR SCIENTIFIC THEORY APPLICATIONS
vs. THE SEMANTIC VIEW OF SCIENTIFIC
THEORIES:
The Case of Models of the Nuclear Structure**

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Abstract:

REPRESENTATION MODELS AS DEVICES FOR SCIENTIFIC THEORY APPLICATIONS *vs.* THE SEMANTIC VIEW OF SCIENTIFIC THEORIES: The Case of Models of the Nuclear Structure

Analyses of the nature and structure of scientific theories have predominantly focused on formalisation. The Received View of scientific theories considers theories as axiomatised sets of sentences. In Hilbert-style formalisation theories are considered formal axiomatic calculi to which interpretation is supplied by a set of correspondence rules. The Received View has long been abandoned. The Semantic View of scientific theories also considers theories as formal systems. In the Semantic conception, a theory is identified with the class of intended models of the formal language, if the theory were to be given such linguistic form. The proponents of the Semantic View, however, hold that this class of models can be directly defined without recourse to a formal language. Just like its predecessor, the Semantic View is also not free of untenable implications. The uniting feature of the arguments in this work is the topic of theoretical representation of phenomena. The Semantic View implies that theoretical representation comes about by the use of some model, which belongs to the class that constitutes the theory. However, this is not what we see when we scrutinise the features of actual representation models in physics. In this work particular emphasis is given to how representation models are constructed in Classical Mechanics and Nuclear Physics and what conceptual resources are used in their construction. The characteristics that these models demonstrate instruct us that to regard them as families of theoretical models, as the Semantic View purports, is to obscure how they are constructed, what is used for their construction, how they function and how they relate to the theory. For instance, representation models are devices that frequently postulate physical mechanisms for which the theory does not provide explanations. Thus it seems more appropriate to claim that these representation devices mediate between theory and experiment, and at the same time possess a partial independence from theory. Furthermore, when we focus our attention to the ways by which representation models are constructed we discern that they are the result of the processes of abstraction and concretisation. These processes are operative in theoretical representation and they demand our attention if we are to explicate how theories represent phenomena in their domains.

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1 Introduction

Much of the work in Philosophy of Science on the nature of Scientific Theories has focused on formalisation issues. This trend prevailed, initially with the Logical Positivist attempt to eliminate metaphysics from science, and reduce philosophy of science to the logical analysis of scientific theories, of their concepts, and of their languages. The logical positivist program eventually led to the view that scientific theories can be formalised in first-order language with identity. A scientific theory, in this view, is identified with a pure syntax (which is expressed in Hilbert-style formalisation as a formal axiomatic calculus) to which an interpretation is supplied at the point of application via a set of correspondence rules. The logical positivist program has long been abandoned and so has this view of the nature and structure of scientific theories. Chapter 2 contains a brief analysis of the logical positivist view of Scientific Theories and a presentation of the major philosophical arguments that led to its demise. It is meant as a historical introduction to the debate about scientific theories.

The successor of the logical positivist view is nowadays referred to as the Semantic View of scientific theories. The Semantic View, just like the Logical Positivist program, is also an attempt to establish a formal reconstruction of theories. The tool employed by the Semantic View, in its quest to analyse the nature and structure of theories, is not first-order formal calculus but model-theory. In the Semantic conception, a theory is identified with the class of intended models of the formal language, if the theory were to be given such linguistic form. The proponents of the Semantic View, however, hold that this class of models can be directly defined without recourse to a formal language. Just like its predecessor, the Semantic View is

also not free of untenable implications. Chapter 3 focuses on the presentation of the Semantic View as advocated by two of its prominent proponents, Bas van Fraassen and Frederick Suppe. It is primarily an attempt to understand the Semantic View and through this exploration to extract its main implications.

The uniting feature of the arguments in this work is the topic of theory application and theoretical representation of phenomena. Emphasis on formal tools leads to a highly idealised, often distorted, understanding of theoretical representation for both the logical positivist and the Semantic views. The Semantic View implies that theoretical representation of phenomena comes about by the use of some model, which belongs to the class of models that constitutes the theory. This model is contrasted or compared to a model of data by some form of structural mapping. However, this is not what we see when we scrutinise the features of actual representation models in physics. In this work particular emphasis is given to how representation models are constructed in Classical Mechanics and Nuclear Physics and what conceptual resources are used in their construction. The characteristics that these models demonstrate instruct us that to regard them as families of theoretical models, as the Semantic View purports, is to obscure how they are constructed, what is used for their construction, how they function and how they relate to the theory.

There are primarily two dimensions to the main argument of the thesis. The negative dimension is an attempt to establish where the Semantic View of scientific theories goes wrong in its description of actual theories. The positive dimension is an attempt to establish that more often than not scientific models are necessary devices for theoretical representation of phenomena. Frequently these models act as intermediaries between theory and experiment and hence it is important to understand how they relate to theory, how they are constructed and how they function.

Chapter 4 is an attempt to explicate how representation models in Classical Mechanics are constructed and to argue that a sharp distinction between models of theory and models of data, as the Semantic View purports, is untenable. Chapters 5 and 6 are devoted to the construction of several representation models in Nuclear Physics. In all three of these Chapters an attempt is made to argue against the

Semantic View, by pointing to the fact that representation models in physics do not have the characteristic features that this view attributes to them. Indeed, representation models are devices that frequently postulate physical mechanisms for which the theory does not provide explanations. In other cases, representation models are constructed by the use of background knowledge, for the sole purpose of providing an explanation of experimental results, and then imported into the theory in ways that the Semantic View offers no help in understanding. Thus it seems more appropriate to claim that these representation devices mediate between theory and experiment, and at the same time possess a partial independence from theory. Chapter 6 is also an attempt to show that the evolutionary history of representation models is obscured, unless we explore the processes of model construction. Furthermore, when we focus our attention to the ways by which representation models are constructed we discern that they are the result of the thought processes that I choose to call *abstraction* and *concretisation*. These processes are operative in theoretical representation and they demand our attention if we are to explicate how theories represent phenomena in their domains, or more precisely, how theories are used in the construction of representation models. Tacit throughout Chapters 4, 5 and 6 is the view that theories and models are constructed via these thought processes. It is not until the last Chapter, however, that I offer a formal schema of a theory of abstraction and concretisation, in an effort to explicate the processes of construction of scientific models as practised in actual science.

2 The Received View of Scientific Theories

2.1 Introduction

What has come to be called -following Putnam (1962)- the *Received View* of Scientific Theories is a view on the nature and structure of scientific theories associated with Logical Positivism. The Received View is nowadays widely considered as inadequate. Nonetheless, a clarification of its major features is important in understanding contemporary views. In briefly presenting and analysing it and most importantly outlining the major philosophical arguments against it, my aim is to facilitate an understanding of the historical picture of the debate about the structure of scientific theories.

The Received View regards scientific theories as axiomatised sets of sentences in mathematical logic, e.g. first-order predicate calculus with identity. The terms of such logical axiomatisations are generally divided into three kinds: (1) logical and mathematical terms, (2) theoretical terms, and (3) observation terms. The scientific laws, which specify relations holding between the theoretical terms, constitute the axioms of the theory. Via a set of *correspondence rules*, theoretical terms are reduced to, or defined by, observation terms. In its history, the Received View underwent several developments, but I think that what Suppe (1974) calls the ‘final version of the Received View’ (which I shall herein refer to as the RV) is a convenient starting point. Since here I am not concerned with the historical development of the RV, an

interesting and philosophically detailed study of which can be found in Suppe (1974), earlier versions of this view are not of particular importance. This version is a convenient starting point because it is the most sophisticated and satisfactory version and hence the least prone to criticism. In Suppe's presentation, the RV construes scientific theories as having '...a canonical formulation satisfying the following conditions:

- (1) There is a first-order language L (possibly augmented by modal operators) in terms of which the theory is formulated, and a logical calculus K defined in terms of L .
- (2) The nonlogical or descriptive primitive constants (that is, the "terms") of L are bifurcated into two disjoint classes:
 - V_O which contains just the observation terms;
 - V_T which contains the nonobservation or theoretical terms.
 - V_O must contain at least one individual constant.
- (3) The language L is divided into the following sublanguages, and the calculus K is divided into the following subcalculi:
 - (a) The *observation language*, L_O , is a sublanguage of L which contains no quantifiers or modalities, and contains the terms of V_O but none from V_T . The associated calculus K_O is the restriction of K to L_O and must be such that any non- V_O terms (that is, nonprimitive terms) in L_O are explicitly defined in K_O ; furthermore, K_O must admit of at least one finite model.
 - (b) The *logically extended observation language*, L_O' , contains no V_T terms and may be regarded as being formed from L_O by adding the quantifiers, modalities, and so on, of L . Its associated calculus K_O' is the restriction of K to L_O' .
 - (c) The *theoretical language*, L_T , is the sublanguage of L which does not contain V_O terms; its associated calculus, K_T , is the restriction of K to L_T .

These sublanguages together do not exhaust L , for L also contains *mixed sentences* that is, those in which at least one V_T and one V_O term occur. In addition it is assumed that each of the sublanguages above has its own stock of predicate and/or functional variables, and that L_O and L_O' have the same stock which is distinct from that of L_T .
- (4) L_O and its associated calculi are given a *semantic interpretation* which meets the following conditions:
 - (a) The domain of interpretation consists of concrete observable events, things, or things-moments; the relations and properties of the interpretation must be directly observable.
 - (b) Every value of any variable in L_O must be designated by an expression in L_O .

It follows that any such interpretation of L_O and K_O , when augmented by appropriate additional rules of truth, will become an interpretation of L_O' and K_O' . We may construe interpretations of L_O and K_O as being *partial semantic interpretations* of L and K , and we require that L and K be given no observational semantic interpretation other than that provided by such partial semantic interpretations.
- (5) A *partial interpretation* of the theoretical terms and of the sentences of L containing them is provided by the following two kinds of postulates: the *theoretical postulates* T (that is, the axioms of the theory) in which only terms of V_T occur, and the *correspondence rules* or postulates C which are mixed sentences. The correspondence rules C must satisfy the following conditions:
 - (a) The set of rules C must be finite.
 - (b) The set of rules C must be logically compatible with T .
 - (c) C contains no extralogical term that does not belong to V_O or V_T .
 - (d) Each rule in C must contain at least one V_O term and at least one V_T term essentially or nonvacuously.' [Suppe 1974, pp50-51]

Following Suppe, let TC be the conjunction of T and C , where T is the conjunction of the theoretical postulates and C is the conjunction of the correspondence rules. Then TC designates the scientific theory that is based on L , T , and C .

The above sketch of the RV contains many features the rationale and implications of which shall not be treated here. Indeed, I shall limit myself only to those features of the RV that are relevant to the main criticisms. The above version of the Received View, as well as previous versions of it, has been criticised *inter alia* on the following grounds: (1) On its reliance on an observation-theory distinction and in addition on an analytic-synthetic distinction. (2) On its employment of a correspondence-rule account of the interpretation of theoretical terms. (3) On its commitment to a theory consistency condition and to a meaning invariance condition. (4) On the fact that it obscures a number of epistemologically important features of scientific theories. (5) On the fact that it assigns a deductive status to empirical theories.

Although the conclusive power of these criticisms is, as will be seen, doubtful, collectively they have been persuasive among philosophers of science and as a result the Logical Positivist analysis of scientific theories gradually gave room to other schools of thought.

2.2 The Observation-Theory Distinction

The separation of L into V_O and V_T terms implies the need of an observation-theory distinction in the terms of the vocabulary of the theory. What might be more difficult to discern is that the rationale for the RV's dependence on the observation-theory distinction is provided by the analytic-synthetic distinction. The analytic-synthetic distinction is embodied in the RV, because (as suggested by Carnap 1956, pp222-229) implicit in TC are meaning postulates that specify the meanings of sentences in L . However, if meaning specification were the only function of TC then TC would be analytic, and in such case it would not be subject to empirical investigation. TC must therefore have a factual component, and the meaning postulates must separate the

meaning from the factual component. This implies an analytic-synthetic separation, since those sentences in L that are logical truths or logical consequences of the meaning postulates are analytic and all non-analytic sentences are synthetic. Furthermore, any non-analytic sentence in L taken in conjunction with the class of meaning postulates, has certain empirical (i.e. L_O) consequences. If the conjunction is refuted or confirmed by directly observable evidence, this will reflect only on the truth-value of the sentence and not on the meaning postulates. Hence the sentence can only be synthetic. The issue could therefore be understood as a need for the RV to characterise meaning postulates for a theoretical language.

Against the observation-theory distinction there are mainly three kinds of criticisms: (1) criticisms aimed to prove the untenability of the analytic-synthetic distinction, (2) attempts to establish accounts of ‘observation’ that are incompatible with a theory-observation distinction, (3) arguments showing that for scientific languages the observation-theory distinction cannot be drawn.

2.2.1 The Untenability of the Analytic-Synthetic Distinction

The main criticism against the analytic-synthetic distinction attempts to show its untenability. In ‘Two Dogmas of Empiricism’, Quine (1951) points out that there are two kinds of analytic statements, (a) logical truths, which remain true under all interpretations, and (b) statements that are true by virtue of the meaning of their non-logical terms, e.g. ‘No bachelor is married’. He then proceeds to argue that the analyticity of statements of the second kind cannot be established without resort to the notion of synonymy. The latter notion, however, is just as problematic as the notion of analyticity.

The argument runs, roughly, from the notion of meaning to the notion of cognitive synonymy and finally to the notion of analyticity. If meaning (or intention) is clearly distinguished from its extension, i.e. the class of entities to which it refers, then the theory of meaning is primarily concerned with cognitive synonymy (i.e. the synonymy of linguistic forms). For example, to say that ‘bachelor’ and ‘unmarried

man' are cognitively synonymous is to say that they are interchangeable in all contexts without change of truth-value. If such is the case then the statement 'No bachelor is married' would become 'No unmarried man is married', which would be a logical truth given the proper logical calculus. In other words, statements of kind (b) are reduced to statements of kind (a) if only we could interchange synonyms for synonyms. But as Quine argues, the notion of interchangeability *salva veritate* is an extensional concept and hence does not help with analyticity. In fact, no analysis of the interchangeability *salva veritate* account of synonymy is possible without recourse to analyticity, thus making such an effort circular, unless interchangeability is '... relativised to a language whose extent is specified in relevant respects' [*Ibid.*, p30]. That is to say, we first need to know what statements are analytic in order to decide which expressions are synonymous, hence appeal to synonymy does not help with the notion of analyticity.

White (1952) gives an argument along similar lines. He argues that an artificial language, L_1 , can be constructed with appropriate definitional rules, in which the predicates P_1 and Q_1 are synonymous whereas P_1 and Q_2 are not; hence making such sentences as $\forall x(P_1(x) \rightarrow Q_1(x))$ logical truths and such sentences as $\forall x(P_1(x) \rightarrow Q_2(x))$ synthetic. In a different artificial language L_2 , P_1 could be defined to be synonymous to Q_2 and not to Q_1 , hence making the sentence $\forall x(P_1(x) \rightarrow Q_2(x))$ a logical truth and the sentence $\forall x(P_1(x) \rightarrow Q_1(x))$ synthetic. This relies merely upon convention. However, he asks, in a natural language what rules are there that dictate what choice of synonymy can be made such that one formula is a synthetic truth rather than analytic? The main point of the argument is therefore that in a natural language or in a scientific language, which are not artificially constructed and which do not contain definitional rules, the notion of analyticity is obscure. Both arguments are far more composite and reach much stronger conclusions; for instance they try to establish that even for artificial languages the notion of analyticity remains obscure.

In arguing for the analytic-synthetic distinction, Carnap and other proponents of the RV were aware of the obscurity of the notion of analyticity in natural languages.¹

¹ See, Creath 1990, pp427-432; Carnap 1956, pp205-221; Putnam 1962a.

Indeed such arguments, as the above, are not conclusive, primarily because the RV is not intended as a description of actual scientific theories. But, as Suppe well recognises, ‘...it presents a canonical linguistic formulation for theories and [it] claims that any theory can be given an essentially equivalent reformulation in this canonical way.’ [Suppe 1972, p3] In other words, the RV is offered as a *rational reconstruction* of scientific theories, i.e. an explication of the structure of scientific theories. It does not aim to describe how actual theories are formulated, but to indicate a logical framework into which theories can be essentially reformulated. Therefore all Carnap, and other proponents of the RV, needed to show was that the analytic-synthetic distinction is tenable in some artificial language (with definitional rules or meaning postulates) in which scientific theories could potentially be reformulated. In view of this, despite the purpose of the above arguments to deny the existence of the analytic-synthetic distinction altogether, what the arguments establish about the RV is something different: The RV requires a clear way by which to characterise meaning postulates for a theoretical language.

Putnam (1962a) is not hesitant to claim that statements of the kind ‘All bachelors are unmarried’ are indeed analytic by virtue of the meaning of their non-logical terms. The important question for him is, how does one clarify the distinction of such statements from synthetic ones? He addresses this question by developing the concept of, what he calls, ‘law-cluster’. A law-cluster concept is constituted by a bundle of laws that collectively determine its identity. Any one law of the collection can be abandoned without destroying the identity of a law-cluster concept. The concept of ‘energy’ exemplifies a law-cluster concept, but there is an abundance of terms in highly developed sciences that are law-clusters. Statements involving law-cluster concepts are neither analytic nor synthetic. On the one hand, there cannot be an analytic statement involving law-cluster concepts because such a statement would be another law in the collection that determines the identity of the concept, and this statement could be abandoned without destroying the identity. And since analytic statements are statements that could not be abandoned without alterations in the meanings of the terms involved, law-clusters are not analytic. On the other hand, Putnam distinguishes law-cluster concepts from synthetic statements by the use of a rather idiosyncratic construal of ‘synthetic’. According to Putnam, synthetic

statements are those that can be refuted by a single observation or experimental test, or be verified inductively by simple enumeration. Tests of statements that involve law-clusters are never tests of an isolated statement. They are always tests of a conjunction of law-statements that determine the identity of the law-cluster and the most one can conclude is the refutation or the verification of the conjunction. Putnam's examples to demonstrate the point are the case of the classical law of kinetic energy ($e = \frac{1}{2}mv^2$, which seems definitional in character) and the laws of Euclidean geometry. Equating the kinetic energy with half the product of the mass and the square of the velocity was given up with the development of the Special Theory of Relativity. However, the extensional meaning of the term kinetic energy (i.e. energy of motion) has not changed, hence it could not have been an analytic truth. Although a new (special relativistic) theory was proposed, the classical $e = \frac{1}{2}mv^2$ was not overthrown just by an isolated experiment, hence it could not have been a synthetic truth. An analogous argument is given for the case of the laws of Euclidean geometry.

Putnam uses this conceptual apparatus to argue that statements of the kind 'All bachelors are unmarried' can safely be decided to be held non-revisable, whereas statements of the kind $e = \frac{1}{2}mv^2$ cannot, because 'energy' is a law-cluster term but 'bachelor' is not. 'This is not to say that there are no laws underlying our use of the term 'bachelor' ... but it is to say that there are no exceptionless laws of the form 'All bachelors are...' except 'All bachelors are unmarried' ...and consequences thereof. Thus preserving the interchangeability of 'bachelor' and 'unmarried' in all extensional contexts can never conflict with our desire to retain some other natural law of the form 'All bachelors are...' [Putnam 1962a, p384] Thus Putnam addresses Quine's and White's arguments, because a sentence of the kind $\forall x(P(x) \rightarrow Q(x))$ is analytic in an artificial language if and only if the interchangeability of Q and P never conflicts with some other natural law, i.e. if P is not a law-cluster concept. Furthermore, since such statements as 'All bachelors are unmarried' do not involve law-cluster concepts they can only be false by altering the meanings of their constituent terms. The concepts involved in such statements are 'fixed points' of the language, strict synonymies with minimal systematic import, i.e. with hardly any theoretical grounds for accepting or rejecting them. By inquiring further into the

rationale for introducing analytic statements, Putnam concludes that they do no harm and that they provide language with intelligibility and practicality.

Putnam presents a way by which the close to our common sense analytic-synthetic distinction can be saved, despite Quine's arguments. However, his argument does affect the RV since it requires that the meaning postulates do not consist entirely of sentences that specify meanings in L , but also of law-cluster concepts. It follows from his argument that many of the sentences in L , which will involve law-clusters, will be neither analytic nor synthetic; hence Carnap's attempt to separate the factual from the meaning content of TC fails.

2.2.2 The Theory-Ladenness of Observation

Attempts to establish accounts of 'observation' that are incompatible with the observation-theory distinction have concentrated mainly on showing the theory-ladenness of observation statements. Hanson's argument is a good example of such. He tries to show that there is no theory-neutral observation language and that observation is 'theory-laden'.² He does this by attempting to establish that an observation language that intersubjectively can be given a theory-independent semantic interpretation, as the RV purports, can not exist.

He begins by asking whether two people see the same things when holding different theories. For example he asks whether Kepler and Tycho Brahe see the same thing when looking at the sun rising. Kepler of course holds that the earth revolves around the sun, whilst Tycho holds that the sun revolves around the earth. Hanson addresses this question by first considering diagrams that sometimes can be seen as one thing and other times as another. The most familiar example of this kind is the duck-rabbit diagram.³

² Hanson 1958, pp4-30. Hanson 1969, pp59-198. Also see Suppe 1974, pp151-166.

³ Many examples of such diagrams can be found in Hanson (1958) and (1969).

Such a diagram can be seen to represent a duck or a rabbit depending on the perspective one takes, but in both cases one sees the same thing. Hanson uses this to develop a sequence of arguments. If the difference in seeing a duck or a rabbit involves interpreting the drawing lines then interpretation is an essential part of seeing, since in this case it contributes to seeing two different things. However he challenges this assertion on the grounds that interpreting is part of thinking, and not an experiential state. One does not first see a duck and then via some process of interpretation then sees a rabbit. On the contrary, the switch from seeing one thing to seeing the other seems to take place spontaneously and moreover a process of back and forth seeing without any thinking seems to be involved. Therefore since interpretation is a form of thinking, Hanson excludes the possibility that it is involved in this process.

He then asks, if interpretation is not involved then what accounts for the difference in what is seen? His answer is that what changes is the organisation of what one sees, meaning that what one sees is appreciated in a different way. The organisation of what one sees depends on the background knowledge and experience of the observer. When Tycho and Kepler look at the sun, they are visually aware of the same object, but they see different things in the sense that their conceptual organisations of their experiences are vastly different. Thus there is a sense in which observation is theory-laden, *viz.* observation is conditional on background knowledge.

Hanson further purports to show that these conceptual organisations are part of the concept of seeing. In science it is important that seeing, unlike picturing, involves a linguistic component. This component enters into 'seeing' because what is relevant to knowledge is 'seeing that...', which is a logically distinguishable element of seeing. A sentence like 'Seeing that...' is always followed by a propositional clause. And without this linguistic component what we observe can have no relevance to our knowledge. By this he tries to point to what is wrong with the RV's sense data position. The RV claims that Kepler and Brahe see the same sense datum, but this only means (according to Hanson) that they picture the same thing, i.e. they perceive the same representation or arrangement. Seeing, however, has a linguistic component so it must involve characterisation or it must have reference. If seeing is understood

to have a linguistic component then it follows that two observers seeing the same thing implies meaning the same thing and not simply asserting that they see the same thing.

By these arguments Hanson attempts to establish that the sense data position is incorrect and also that conceptual organisations are logical features of ‘seeing’, which are indispensable to scientific observation. It is, however, questionable whether Hanson’s arguments are conclusive. They can be disputed, for instance, on the grounds that conceptual organisation is part of the process of interpretation. When looking at the diagram we do see the same thing, if we have the concepts of duck or rabbit we apply the concept to what we see, but unless we do have the concepts of duck or rabbit we just see the diagram. Although his arguments do not tackle the tenability of the observation-theory distinction conclusively, they do nevertheless provide a persuasive consideration that observation is theory-laden.⁴

2.2.3 The Untenability of the Observation-Theory Distinction

Achinstein’s and Putnam’s⁵ objections to the observation-theory distinction are twofold. On the one hand, they claim that an observation-theory distinction of scientific terms cannot be drawn. And on the other, that a classification of terms following the above distinction gives rise to a distinction of observational-theoretical statements. The latter distinction can also not be drawn for scientific languages.

Achinstein’s argument is that the sense of the term ‘observation’ relevant to science involves visually attending to something. He assigns to the scientific sense of observation the following characteristics [Achinstein 1968, pp160-165]: (1) It involves attention to the various aspects or features of an item depending on the

⁴ Hanson continues his argument to establish that not only is observation theory-laden but that so are facts and causality. This way he attempts to show that there does exist a logic of discovery contrary to the claim of the RV that only the context of justification belongs to the realm of the Philosophy of Science, whereas the context of discovery belongs to the domains of History and Psychology.

⁵ Achinstein 1965, 1968. Putnam 1962.

observer's concerns and knowledge. (2) It does not necessarily involve recognition of the item. (3) Observing something does not imply that it is in the visual field or in the line of sight of the observer, e.g. a distant fire. (4) Observing an item could be done indirectly, for instance looking at a mirror image. (5) The description of what one observes can be done in different ways.

If now one urges an observation-theory distinction by simply presenting lists of observable and unobservable terms (as proponents of the logical positivist view, according to him do), the distinction could be objected to. For instance according to typical lists of unobservables, 'electrons' and 'fields' are theoretical terms. But based on points (3) and (4) above, Achinstein claims that, this could be rejected. Similarly based on point (5), he also rejects a statement distinction, because 'what scientists as well as others observe is describable in many different ways, using terms from both [the theoretical, V_T , and the observational, V_O] vocabularies.' [*Ibid.*, p165]

Logical positivists have claimed that items in the observational list are directly observable whereas those in the theoretical list are not (and they have also attached importance to the number of observations necessary in order to claim that an item belongs to the observational list).⁶ Achinstein's claim is that once 'directly observable' is closely construed, the desired classification of terms into the two lists fails. His consideration is that 'directly observable' could mean that it can be observed without the use of instruments or by only observing something distinct from it. If this is what Carnap and Hempel have in mind then it does not warrant the distinction. First, it is not precise enough to classify things seen by images and reflections, e.g. a cell nucleus. Second, if something is not observable without instruments means that no aspect of it is observable without instruments then things like temperature, mass, charge, entropy, would be observables, since some aspects of them are detected without instruments. If however it means that instruments are required to detect its presence, then it is insufficient because one cannot talk about the presence of temperature, or kinetic energy. Finally, if it means that instruments are required to measure it or its properties, then such terms as volume, water, weight,

⁶ See, Hempel 1958, Carnap 1936-37 and 1956a.

etc., would be theoretical terms. Hence, Achinstein concludes that the notion of direct observability fails to draw the desired observation-theory distinction, although he does realise that it may give rise to a whole lot of other distinctions.⁷

Putnam's claim is that the distinction is completely 'broken-backed' mainly for three reasons. Firstly, if an observation term is one that only refers to observables then there are no observation terms. For example the term 'red', which is in the observable class, but which was used by Newton to refer to a theoretical term, namely red corpuscles. Secondly, many terms that refer primarily to the class of unobservables are not theoretical terms. Thirdly, some theoretical terms, that are of course the outcome of a scientific theory, refer primarily to observables. For example Darwin's theory of evolution, as originally put forward, referred to observables by employing theoretical terms.

The most that these arguments accomplish is to point to the fact that scientific languages employ terms that cannot clearly and easily be classified into observational or theoretical. They do not however show the untenability of the observation-theory distinction as employed by the RV. In fact, as Suppe (1972) argues, what the RV needs is an artificial language for science, no matter how complex it may turn out to be. Such a language, in which presumably the observation-theory distinction is tenable, must have a plethora of terms and concepts. Such that, to use his example, the designated term 'red_o' will refer to observable occurrences of the predicate red, and the designated term 'red_i' will refer to unobservable occurrences. I think this example is indicative of ways out of the objections raised by Achinstein and Putnam.

2.3 Correspondence Rules

Logical positivists were determined to distinguish the character and function of theoretical terms from speculative metaphysical ones (such as, 'unicorn', 'ghost', 'holy spirit'). In their efforts to establish such a distinction, they sought a kind of

⁷ Achinstein 1968, pp172-177.

‘connection’ of theoretical to observational terms, thus providing an analysis of the empirical nature of theoretical terms contrary to that of metaphysical terms. This ‘connection’ was formulated in what I shall call, following Achinstein (1963), the *thesis of Partial Interpretation*. The thesis of partial interpretation is basically the following: Clauses (4) and (5) of the RV allow that a complete empirical semantic interpretation in terms of directly observables is given to V_O terms and to sentences in L_O and L_O' . However, no such interpretation is intended for V_T terms and consequently for sentences of L containing them. The empirical or observational content of theoretical terms is supplied by TC as a whole. Such terms receive a partial observational meaning indirectly by being related to sets of observational terms, via certain postulates. These postulates are known as *correspondence-rules*. To use one of Achinstein’s examples, ‘it is in virtue of [a correspondence-rule] which connects a sentence containing the theoretical term ‘electron’ to a sentence containing the observational term ‘spectral line’ that the former theoretical term gains empirical meaning within the Bohr theory of the atom.’ [Achinstein (1963), p90]

Correspondence-rules were initially introduced to serve three functions in the RV: (1) to define theoretical terms, (2) to guarantee the cognitive significance of theoretical terms, and (3) to specify the admissible experimental procedures for applying theory to phenomena. In the initial stages of Logical Positivism it was held that, on the basis that observational terms were cognitively significant, theoretical terms were cognitively significant if and only if they were explicitly defined in terms of observational terms. The criteria of explicit definition and cognitive significance were abandoned once Carnap became convinced that dispositional terms, which are cognitively significant, do not admit of explicit definitions.⁸ Consider the dispositional term ‘tearable’ (let us assume all the conditions necessary for an object to be torn apart hold), if we try to explicitly define it in terms of observables we end up with something like this:

An object x is tearable if and only if, if it is pulled sharply apart at time t then it will tear at t (for simplicity let us ignore time lapse for a material to be torn apart).

⁸ Carnap 1936-37. Also see Hempel 1952, pp.23-29, and Hempel 1958.

We can render the above definition as: $\forall x(T(x) \leftrightarrow \forall t(P(x,t) \rightarrow Q(x,t)))$. Where, T is the theoretical term ‘tearable’, P is the observational term ‘pulled apart’, and Q is the observational term ‘tears’. But this does not define the actual dispositional property *tearable*, because the right-hand side of the biconditional will be true of objects that are never pulled apart. As a result some objects that are not tearable and have never being pulled apart will by definition have the property ‘tearable’.

As a result Carnap proposed to replace the construal of correspondence-rules as explicit definitions, by *reduction sentences* that partially determine the observational content of theoretical terms. A *bilateral* kind of reduction sentence would define the dispositional property tearable as: $\forall x \forall t(P(x,t) \rightarrow (Q(x,t) \leftrightarrow T(x)))$. Unlike the explicit definition case, if a is a non-tearable object that is never pulled apart then it is not implied that $T(a)$ is true. What will be implied is that $\forall t(P(a,t) \rightarrow (Q(a,t) \leftrightarrow T(a)))$, is true. Thus the defect of explicit definitions is avoided, because a reduction sentence does not completely define a disposition term. In fact this is also the reason why correspondence-rules supply only partial observational content, since many other reduction sentences can be used to supply other empirical aspects of the term *tearable*, e.g. being torn by excessively strong shaking. Finally as a result of abandoning the criterion of explicit definition, the criterion of cognitive significance was also abandoned and replaced by that of empirical significance. Empirical significance is tightly connected to correspondence-rules understood as reduction sentences. A term has empirical significance if it can be introduced through chains of true reduction sentences, on the basis of observation terms.⁹

The status of correspondence-rules in the RV is therefore simply to contribute (as part of TC) to the partial interpretation of theoretical terms and languages. It must be noted that TC specifies the interpretation of V_T terms only in the strict sense of observational interpretation. A full interpretation of V_T terms can be specified through a richer meta-language. Thus the RV accommodates the general intuition that a V_T term has various non-empirical associations that may contribute to its meaning and

⁹ For the changes in the use of correspondence-rules through the development of the Received View, see Suppe 1974, pp17-27.

that only part of the full meaning of V_T terms is empirical. Furthermore, partial interpretation in this sense is all the RV needs since, given its goal of distinguishing theoretical from speculative metaphysical terms, it only requires a ‘connection’ of the V_T terms to the V_O terms.

The thesis of partial interpretation came under attack from Putnam (1962) and Achinstein (1963 and 1968) who posed the following question: How is the statement ‘ TC provides V_T terms with partial interpretation’ to be understood? Putnam gave the following plausible explications: (1) Using the notion from mathematical logic, to partially interpret V_T terms is to specify a class of intended models with at least two members. (2) To partially interpret a term is to specify a verification-refutation procedure that applies only to a proper subset of the extension of the term. (3) To partially interpret a formal language L is to interpret only part of the language (e.g. to provide translations into common language for some terms and leave the others as mere dummy symbols).

Achinstein gave the following plausible explications: (1') To partially interpret a term is to say that although it has a complete meaning only part of that meaning has been given. (2') To partially interpret a term X means that there are no observational conditions all of which are logically necessary and whose conjunction is logically sufficient for X , but there are other sorts of analytic statements relating X to observational terms. (3') A term X is partially interpreted if, among the sentences in which it appears in the theory there is none of the form $X(a) \leftrightarrow Y(a)$, where $Y(a)$ is an interpreted observational sentence (i.e. from L_O or L_O'), which is not analytic.

Both Putnam and Achinstein offer arguments in an effort to show that for each of their plausible construals, the notion of ‘partial interpretation’ either is inadequate for the RV or is incoherent. It is my understanding that their arguments do not convincingly meet their purpose for all construals of the notion. As this is a side issue to the present discussion I choose not to divert in order to show why this is so.¹⁰ What is important to point out in the context of our discussion is that even if their

¹⁰ An interesting assessment of these arguments may be found in Suppe 1974, pp86-95.

arguments were incorrect or inconclusive, the thesis of partial interpretation evidently presupposes the observation-theory distinction. Therefore, (to say the least) the obviously problematic distinction affects the tenability of the thesis of partial interpretation.

We saw how the first two functions of correspondence-rules, namely providing explicit definitions and cognitive significance to V_T terms, were abandoned and substituted by reduction sentences and partial interpretation. What about the third function, that of specifying the admissible experimental procedures for applying the theory to phenomena or the various sorts of correspondences holding between theory and observation. Suppe (1974, pp102-109) argues that the account of correspondence-rules inherent in the RV is inadequate (for the purposes of understanding actual science) on the following three grounds: (1) They are mistakenly viewed as components of the theory rather than as *auxiliary hypotheses*. (2) The sorts of connections (e.g. explanatory causal chains) that hold between theories and phenomena are inadequately captured. (3) They oversimplify the ways in which theories are experimentally applied to phenomena.

The first criticism is that the RV considers TC as axioms of the theory. Hence C is an integral part of the theory. So if a new experimental procedure is discovered it would have to be incorporated into C , thus resulting in a new set of rules C' , and consequently in a new theory TC' . But obviously the theory has not undergone any change, rather we have just improved our knowledge of how to apply it to phenomena. So we must think of correspondence-rules as auxiliary hypotheses. This, as he admits, is not incompatible with the thesis of partial interpretation. When C was regarded as providing explicit definitions then they did form an integral part of the theory, but once explicit definition is given up it is no longer necessary to construe C as a component of the theory.

The second criticism is based upon Schaffner's (1969) consideration that there is a way in which theories are applied to phenomena, which is not captured by the RV's account of correspondence-rules. This is the case when various theories from outside

T are borrowed and used to describe a 'causal sequence' which obtains between the states described by T and the observation reports. These causal sequences are the descriptions of the mechanisms involved whereby particular states of the physical systems cause the measurement apparatus to behave as it does. Thus they supplement theoretical explanations of the observed behaviour of the apparatus by causally linking TC to the observation reports. If this use of C is recognised then it is best that they are dissociated from the core theory and be regarded as auxiliary hypotheses. For example, such auxiliary hypotheses are used to establish a causal link between the motion of an electron (V_T term) and the spectral line (V_O term) in a spectrometer photograph, or the influence of the molecules of a gas (V_T term) on the gas pressure (V_O term). The point Schaffner is making is that the relation between theory and observation reports comes about by the use of these auxiliary hypotheses. The latter are frequently used to establish explanations of the behaviour of physical systems by linking the theoretical predictions to observational reports via causal mechanisms, hence they are themselves scientific laws. Without recognising the use of these auxiliaries the RV may only describe a type of theory application whereby theoretical states are just correlated to observational states.

Finally, the third criticism is based on Suppes' (1962, 1967) analysis of the complications involved in relating theoretical predictions to observation reports. As an example of what Suppes claims takes place, consider that theoretical predictions are typically predictions derived from continuous functions. An observation report however is a set of discrete data. Now in order to reach the point where the two can be compared several modifications take place on the side of the observation report. For instance, the theory's predictions may be based on the assumption that certain idealising conditions hold, e.g. no friction. Assuming that in the actual experiment these conditions did not hold, it would mean that to achieve a reasonable comparison the observational data will have to be converted into a corresponding set that reflects the result of an 'ideal' experiment. In other words, the actual observational data must be converted into what they would have been had the idealising conditions obtained. This conversion, Suppes argues, comes about by employing appropriate 'theories of data'. So, regularly, there will not be a direct comparison between theory and observation, but a comparison between theory and observation in conjunction with

theory of data. Suppes' analysis is more complicated and far more detailed and we shall encounter it in subsequent Chapters, as it is an integral part of the Semantic View of scientific theories. For the moment what has been said is adequate to make his point: actual scientific practice, and in particular theory application, is far more complex than the description given by the RV's account of correspondence-rules.

2.4 Theory Consistency and Meaning Invariance

Feyerabend criticised the Logical Positivist picture of scientific theories on the grounds that it imposes a *meaning invariance condition* and a *consistency condition* on them. By the consistency condition he meant that, '...only such theories are ... admissible in a given domain which either *contain* the theories already used in this domain, or which are at least *consistent* with them inside the domain.' [Feyerabend 1965, p164] By the condition of meaning invariance he meant that, '...meanings will have to be invariant with respect to scientific progress; that is, all future theories will have to be framed in such a manner that their use in explanations [or reductions] does not affect what is said by the theories, or factual reports to be explained.' [*Ibid.*, p164] Feyerabend's criticisms are not aimed directly at the RV, but rather at two other claims of Logical Positivism, namely the theses of *the development of theories by reduction* and *the covering law model of scientific explanation*; both of which are intimately connected to the RV.

A brief digression, in order to look into the aforementioned theses, would be helpful. Briefly stated, there are two types of development of theories by reduction. First, there is a development of a scientific theory, TC , by expanding its domain to include new systems or phenomena. Expanding the set of theoretical postulates T to T' could accomplish this. Thus the original theory TC is said to be reduced to $T'C$.¹¹ Examples of such a kind of theory reduction are the expansion of classical particle mechanics to

¹¹ We have seen that within the partial interpretation account of correspondence-rules, expanding the set C do not necessarily imply viewing the result as a new theory, hence changes in C does not relate to such kinds of reduction.

the mechanics of rigid bodies, and the ‘absorption’ of Galileo’s laws into Newtonian mechanics and gravitational theory. The main characteristics of such reductions are that the descriptive terms employed in both theories have approximately the same meanings, and that the domains of the two theories are qualitatively homogeneous.¹² A second type of development involves the reduction of one theory (secondary) into a second more inclusive theory (primary). In such kinds of reduction the theory may employ ‘...in its formulations ... a number of distinctive descriptive predicates that are not included in the basic theoretical terms or in the associated rules of correspondence of the primary... [theory]’ [Nagel 1979, p342]. That is to say, the V_T terms of the secondary theory are not necessarily all included in the primary theory. Nagel is mainly concerned with this type of reduction, and he builds up his case based on the example from the history of physics of the reduction of Thermodynamics to Statistical Mechanics. Indeed, he claims that reductions of the first type may be regarded as a special case of the second type, though he does not pursue this claim further. There are several requirements that have to be satisfied for this type of theory reduction to take place, two of which are: (1) The V_T terms for both theories involved in the reduction must have unambiguously fixed meanings by codified rules of usage or by established procedures appropriate to each discipline, e.g. theoretical postulates or correspondence rules. (2) For every V_T term in the secondary theory that is absent from the theoretical vocabulary of the primary theory, assumptions must be introduced which postulate suitable relations between these terms and traits represented by theoretical terms in the primary theory. Furthermore, with the help of these assumptions all the laws of the secondary theory are logically derivable from the primary theory.¹³

The covering law model of scientific explanation (or Deductive-Nomological explanation) is, epigrammatically, explanation in terms of a deductively valid argument. The sentence to be explained (explanandum) is a deductive consequence of

¹² See Nagel 1979, pp336-339.

¹³ See Nagel 1979, pp345-358. Although Nagel presents a larger set of conditions that have to hold in order for reduction to take place (pp. 336-397), these are the two only relevant for the purpose of the argument presented here.

a set of law-premises together with a set of premises consisting of initial conditions or other particular facts involved (explanans). For the special case when the conclusion is a scientific theory, T' , the covering law model can be formulated as follows: a theory T explains T' if and only if T together with initial conditions constitute a deductively valid inference with consequence T' . In other words, if T' is derivable from T together with initial conditions then T' is explained by T . It is patent that reduction and explanation of theories go hand in hand. If T' is reduced to T , then T explains T' and conversely.¹⁴

Feyerabend points out that Nagel's two assumptions -(1) and (2) above- for theory reduction respectively impose a condition of meaning invariance and a consistency condition to scientific progress. The thesis of development of theories by reduction imposes on science that it restricts itself to theories that are mutually consistent. But the consistency condition requires that terms in the admissible theories for the domain must be used with the same meanings. Similarly it can be shown that the covering law model of explanation also imposes these two conditions. In fact the consistency condition follows from the requirement that the explanandum must be a logical consequence of the explanans. Since the meanings of the terms and statements in a logically valid argument must remain constant, an obvious demand for explanation is that meanings must be invariant.¹⁵ Feyerabend objects to both of these conditions and argues his case by drawing examples from the history of scientific progress. For example, the concept of mass does not have the same meaning in relativity theory as it does in classical mechanics. Relativistic mass is a relational concept between an object and its velocity, whereas in classical mechanics mass is a monadic property of an object. Similarly, Galileo's law asserts that acceleration due to gravity is constant, but if Newton's law of gravitation is applied to the surface of the earth it yields a variable acceleration due to gravity. Hence Galileo's law cannot be consistently

¹⁴ This overlap between reduction and explanation is the reason why Feyerabend's arguments are indiscriminately sometimes directed against the development of theories by reduction and other times against the covering law model.

¹⁵ The condition of meaning invariance is also entailed by RV's requirement for a theory-neutral observation language.

derived from Newton's law. Thus he establishes that neither meaning invariance nor the intimately related notion of theory consistency, characterise actual science and scientific progress.¹⁶

2.5 The Received View Obscures Epistemologically Important Features of Scientific Theories

The objection that the RV obscures several epistemologically important features of scientific theories is implicitly present in all versions of the Semantic View of theories. Suppe, however, brings this out explicitly in the form of a criticism.¹⁷ To clarify the sort of criticism presented by Suppe we must refer to an alternative picture of scientific theories, the Semantic View. Since I shall occupy myself with the presentation of the Semantic View in the next Chapter, I shall try to clarify the criticism with only a brief account of some features of this alternative view.

The rationale behind Suppe's argument is the following. It is patent that science has managed, so far, to go about its business without involving the observation-theory distinction and all the complexities that it gives rise to. Thus, he suggests, the distinction is not required or presupposed by science; it must be extraneous to an adequate analysis of scientific theories. So the important question for him concerns not the adequacy or not of an analysis of scientific theories that employs the distinction, i.e. the issue on which many of the criticisms of the RV have focused, but whether the observation-theory distinction '...is required for an adequate analysis of the epistemological structure of theories.' [Suppe 1972, p9]

¹⁶ See Feyerabend 1962, 1963, 1965, 1970, 1981. Feyerabend's views have been criticised by numerous authors. For example objections to his views have been raised based on his peculiar analysis of 'meaning', on which his position relies. His views are hence not presented here as conclusive criticisms of the RV; but simply that they, to say the least, cast doubt on the adequacy of the theses of theory development by reduction and the covering law model of explanation.

¹⁷ See Suppe 1972, 1977, 1989 (chapter 2, in which Suppe 1972 is incorporated).

By relying and expanding on Suppes' analysis of 'theory of data', he argues that because of its reliance on the observation-theory distinction, the RV employs correspondence-rules in such a way as to blend together disparate aspects of the scientific enterprise. Such aspects are the design of experiments, the interpretation of theories, the various calibration procedures, the employment of results and procedures of related branches of science, etc. All these 'disparate aspects' are being lumped into the correspondence-rules. Although these features can be clarified by relevant changes in the correspondence-rule account, he claims that even if this is achieved the correspondence-rule account will still obscure epistemologically important features of scientific theorising.

In applying a theory to phenomena, contrary to the implications of the RV, we do not have any direct link between theoretical terms or entities and observational terms or entities. In a scientific experiment we collect data about the phenomena, and often enough the process of collecting the data involves rather sophisticated bodies of theory. Experimental design and control, instrumentation and reliability checks are necessary for the collection of data. Moreover, sometimes generally accepted laws or theories are also employed in collecting these data. All these features of experimentation and data collection are then employed in ways as to structure the data into forms (which he calls, 'hard data') that allow the application of the theory. In fact, theory application according to Suppe involves contrasting or comparing theoretical predictions to these 'hard data', and not to something directly observed. 'Accordingly, the correspondence rules for a theory should not correlate direct-observation statements with theoretical statements, but rather should correlate 'hard data' with theoretical statements.' [*Ibid.*, p11] Suppe admits that this could potentially be built into the correspondence-rules, but he claims that such changes cannot be done without obscuring epistemologically important features of scientific theorising.

The sciences, he argues, do not deal with all the complexities of phenomena. Rather they isolate a certain number of physical parameters by abstractions and idealisations.

He maintains that these parameters are used to characterise *physical systems*,¹⁸ which are highly abstract and idealised replicas of phenomena. A classical mechanical description of the earth-sun system of our solar system, would not deal with the actual system, but with a physical system in which some relevant parameters are abstracted (e.g. mass, displacement, velocity) from the complex features of the actual system. And in which some other parameters are ignored (e.g. the intensity of illumination by the sun, the presence of electromagnetic fields, the presence of organic life). In addition, the physical system does not involve these abstracted parameters in their full complexity. Indeed it idealises the system by ignoring certain factors or features of the actual system, that may plausibly be causally relevant to these parameters and the actual system as a whole. For instance, it may assume that the planets are point masses, or that their gravitational fields are uniform, or that there are no disturbances to this system by external factors and that the system is in a vacuum. What scientific theories do is attempt to characterise the behaviour of such physical systems not the behaviour of directly observable phenomena.

Although this is admittedly a rough sketch of the actual argument, it is not hard to see that the aim of the argument is to lead to the conclusion that the raw phenomena are connected to a scientific theory via the physical system. That is to say, the connection between the theory and the phenomena is a comparison between a physical system and the 'hard data'. The foundation of this connection requires an analysis of theories and theory-applications that involves a two-stage move. The first move involves the connection between raw phenomena and the 'hard data' about the particular physical system in question. The second move involves the connection between the physical system and the theoretical postulates, etc., of the theory. In this picture we see that the physical system plays the intermediate role between phenomena and theory. This role is what is operative, for Suppe, in illuminating several epistemological features of scientific theorising. It is important that we note that the correspondence-rules '... amalgamate together the two sorts of moves ... so as to eliminate the physical system.' [*Ibid.*, p16]

¹⁸ It will be explained in Chapter 3 that Suppe's use of the term 'physical system' is related to the notion of what logicians would refer to as a 'semantic model'.

The second move usually involves a deductive connection from the theory to the physical system. What is required in addition to the postulates of the theory, in order to accomplish this connection, are boundary and initial conditions. No additional correspondence-rules are required. The first move is much more complex than what the RV is willing to admit. The transition from the phenomena to the physical system reduces, *inter alia*, to ‘problems of measurement’, ‘experimental design’, ‘interpretation and correction of raw data’, ‘employment of theories from other branches of science’, ‘counterfactual claims of the sort: had these idealised conditions been met the phenomena would have behaved in such and such ways’. So, according to Suppe, correspondence-rules must give way to this two-stage transition, if we are to distinguish the epistemic features of physical systems. I concede to Suppe that he discerns essential features of scientific theorising (at a time when they were entirely absent from philosophical debate), such as abstraction and idealisation, which do indeed indicate that the RV obscures epistemological features of scientific theories. However, as we shall see in subsequent Chapters, his proposal falls short of satisfactorily addressing them.

2.6 Hempel’s Proviso Argument

In one of his last writings, Hempel warns against axiomatisations of theories in first-order language and more generally objects to analyses of theories through formalisations. Pertaining to the inferential function of theories, he raises two kinds of problems that impair the deductive status of empirical theories.¹⁹ He assumes that a theory is construed as an ordered pair that consists of a set T of the basic principles of a theory, and a set C of correspondence rules (he uses the term ‘interpretative statements’ or ‘bridge principles’). As understood so far in this Chapter, let the sentences or formulas of T be formulated by the use of a theoretical vocabulary V_T ,

¹⁹ Hempel, (1988). His immediate intention is not to distance himself from Logical Positivism, but to maintain a more liberal view of theories from that of the RV analysed in this chapter.

and the sentences about the phenomena be formulated by the use of an observational vocabulary V_O .²⁰ Finally, let the correspondence rules provide partial interpretation of the terms in V_T by means of the terms in V_O .

Consider now Hempel's example. If we try to apply the theory of magnetism for a simple case we are faced with the following inferential situation. From the sentence ' b is a metal bar to which iron filings are clinging' (S_{O1}), by means of a suitable correspondence rule we infer ' b is a magnet' (S_{T1}). Then by using the theoretical principles in T , we infer 'If b is broken into two bars b_1 and b_2 , then both are magnets and their poles will attract or repel each other' (S_{T2}). Finally using further correspondence rules we derive the sentence 'If b is broken into two shorter bars and these are suspended, by long thin threads, close to each other at the same distance from the ground, they will orient themselves so as to fall into a straight line' (S_{O2}).²¹ Thus, Hempel attributes a basic structure to theoretical inferences: $S_{O1} \xrightarrow{c} S_{T1} \xrightarrow{T} S_{T2} \xrightarrow{c} S_{O2}$, (where the notation $P \xrightarrow{Q} R$ indicates that R is inferred from P by using sentences from Q). If the inferential structure is indeed deductive then it can be read as follows: S_{O1} in combination with the theory deductively implies S_{O2} . This, as Hempel points out, is tantamount to saying that the theory deductively implies the conditional sentence $S_{O1} \rightarrow S_{O2}$ in V_O . Hempel concludes that this deductivist construal faces two difficulties which he calls '...the problem of theoretical [or inductive] ascent and the problem of provisos.' [*Ibid.*, p21]

Let us look at how he explicates the first problem. In the foregoing inferential structure, the first inferential step presupposes that with the help of correspondence rules, S_{T1} is deducible from S_{O1} . However, a search into the theory of magnetism will yield no general principle that, whenever iron filings cling onto a metal bar then the

²⁰ In an effort to avoid the complications caused by the observation-theory distinction, Hempel does not talk of observational vocabulary. Instead he makes use of the notion of 'antecedently understood' vocabulary, which may also consist of theoretical terms that are available and understood independently of the particular theory. It makes no difference to this argument what notion is employed.

²¹ See Hempel 1988, p20.

bar is a magnet. In fact, the bar may be made of lead and covered by an adhesive thus iron filings will cling to it, and the theory does not preclude such possibilities. In short, the theory does not warrant a deduction from S_{O1} to S_{T1} . Hempel concludes:

‘Hence, the transition from S_{O1} to S_{T1} is not deductive even if the entire theory of magnetism is used as an additional premise. Rather, the transition involves what I will call *inductive* or *theoretical ascent*, that is, a transition from a data sentence expressed in V_O to a theoretical hypothesis S_{T1} that would explain, by way of the theory of magnetism, what the data sentence describes.’ [*Ibid.*, p22]

To clarify the second problem, that of provisos,²² it is best to look into the third inferential step from S_{T2} to S_{O2} . What is necessary here is for the theory of magnetism to provide correspondence rules that would turn this step into a deductive inference. The theory however clearly does not do this. In fact, the theory allows for the possibility that the magnets orient themselves in a way other than a straight line, for example if a strong magnetic field of suitable direction is present. This consideration leads to the recognition that the third inferential step presupposes the additional assumption that there are no disturbing influences to the system of concern. Hempel uses the term ‘provisos’, ‘...to refer to *assumptions* ...[of this kind]..., *which are essential, but generally unstated, presuppositions of theoretical inferences*.’ [*Ibid.*, p23] Provisos are also presupposed in the inferential step from S_{T1} to S_{T2} , ‘... for if the breaking of the magnet takes place at a high temperature, the pieces may become demagnetised.’ [*Ibid.*, p23] Therefore, provisos are not just presupposed in the application of a theory, but also in ostensibly deductive inferences within the theory, i.e. from one V_T sentence to another.

What is the character of provisos? Hempel argues that they cannot be viewed as just *ceteris paribus* clauses, as they do not call for the equality of certain things in a vague and elusive manner, instead they call for the absence of disturbing factors. So he suggests we may view provisos as *assumptions of completeness*. For example, in a theoretical inference from one sentence S_1 to another S_2 , a proviso is required that asserts that in a given case ‘...no factors other than those specified in S_1 are present that could affect the event described by S_2 .’ [*Ibid.*, p29] As for example is the case in

²² Because I shall use this argument in a later chapter, in a different context and for a different purpose, I shall go through it in length.

the application of the Newtonian theory to a two star system, where it is presupposed that their mutual gravitational attraction are the only forces the system is subjected to. It is evident that:

‘...a proviso as here understood is not a clause that can be attached to a theory as a whole and vouchsafe its deductive potency by asserting that in all particular situations to which the theory is applied, disturbing factors are absent. Rather, a proviso has to be conceived as a clause that pertains to some particular application of a given theory and asserts that in the case at hand, no effective factors are present other than those explicitly taken into account.’ [Ibid., p26]

An immediate implication is that laws cannot be written down in an explicit way because they are subject to an indefinitely large number of provisos. In attempting to formulate the number of restrictions (implicit in the provisos) we run the risk of reducing the law to a trivial statement. Hence Hempel seems to imply that our only rational choice is to accept the incomplete character of scientific laws. Moreover, Hempel cautions us as not to confound provisos, *viz.* assumptions of completeness, with epistemic requirements for complete and total evidence:

‘A proviso ...calls not for epistemic but for ontic completeness: the specifics expressed by S_1 must include not all the information available at the time (information that may well include false items) but rather all the factors present in the given case that in fact affect the outcome to be predicted by the theoretical inference. The factors in question might be said to be those that are ‘nomicallly relevant’ to the outcome, that is, those on which the outcome depends in virtue of nomic connections.’ [Ibid., p29]

Let us look into his example closely. By the use of the Newtonian theory T we try to infer from S_1 a sentence S_2 . Hempel proposes that this inference can be schematised as $(P \wedge S_1 \wedge T) \rightarrow S_2$, where P is the proviso that all the influences the system of the two stars is subjected to are specified in S_1 . Thus for the chosen system, P must imply the absence of not just other mechanical forces, but also of electric, magnetic and other forces that may influence the system. Hempel wonders ‘...whether this proviso can be expressed in the language of celestial mechanics at all, or even in the combined languages of mechanics and other physical sciences.’ [Ibid., p30] A scientific theory gives an account of a certain domain of empirical phenomena, so for instance the Newtonian theory of gravitation does not assert or deny the existence of nongravitational forces. This may *prima facie* lead to the conclusion that provisos transcend the conceptual resources of the theory. But as Hempel points out, this is not the case for the above example, since in Newton’s 2nd law $F=ma$ stands for the total force on the body. Thus the proviso can be expressed in the language of the theory:

the total force exerted on each of the two bodies is due to the gravitational force exerted upon one by the other, and that this force is determined by the law of gravitation. But in applying the theory to particular cases it must again be subject to provisos, to the effect that all relevant factors affecting the system have been accounted for in the computation of the total force.

We see that Hempel's challenge is that theoretical inferences are not deductively valid because they presuppose provisos. We can also see that in going from observed facts to theoretical claims, the move is not deductive but it involves a theoretical or inductive ascent. Both of these features of theories impair the status of two obvious implications of the RV: (1) that theories are deductively connected sets of statements that stretch all the way to the phenomena and (2) that some of these statements, the laws of the theory, are empirical universal generalisations.

2.7 General Remarks on the Received View

The RV is intended as an explicative and not a descriptive picture of scientific theories. We have seen that even as such it is vulnerable to a great deal of criticism. Although I have not presented the major arguments against the RV with the intention of discrediting it altogether, one cannot help wondering whether even if the RV was formulated in such a way as to avoid the preceding criticisms would the result aid our understanding of science in any significant way? During the last few decades Philosophy of Science has made an important methodological turn. In the quest to provide a *description* of scientific theories, it is now occupied with the study of actual science as practised by working scientists and not some rational reconstruction of theories.

The RV construes scientific theories as being canonically formalisable. Nothing was said about formalisation issues and it will not be taken up now. Nevertheless, what is worth mentioning is that the RV identifies theories with their linguistic formulations, i.e. collections of propositions that can be axiomatised in a first-order language with

identity. Attention to the syntactic character of the RV was not given because it is an element that does not belong exclusively to the RV. The demise of the RV has given rise to the Semantic View of theories, which eliminates the role of language from theoretical representation of phenomena. Theories are, according to the Semantic View, classes of mathematical structures, and theoretical representation is accomplished by matching one of these structures to the data. Therefore the Semantic View is an attempt to retain formalisation as the major tool of analysis of scientific theories. This is done by shifting the emphasis from proof-theory to model-theory, at the expense of the representational character of language. It is common, however, to regard theories as languages constituted by interpreted collections of statements without divorcing logical syntax from meaning as the RV suggests. Through the latter understanding of theories, language (and in particular mathematical equations) is understood as one of the means of theoretical representation. It is with this sort of syntactic spirit that I now turn to critically assess the Semantic View of theories.

3 The Semantic View of Scientific Theories

3.1 Introduction

The overwhelming weaknesses of the Received View together with the developments in Set Theory, particularly by the Bourbaki attempt to develop a set theoretical representation of the entire of mathematics, *inter alia* led to a new school of thought regarding the nature of Scientific Theories. This school was initiated and motivated by the work of Patrick Suppes, which when taken up by other philosophers branched out into two directions. These came to be named the Semantic View of Theories (associated with the work of van Fraassen, Suppe, Giere and others) and the Structuralist Approach (associated with the work of Sneed, Stegmüller, Moulines, Balzer and others). Although the two labels are improvised inventions and are not widely regarded as accurate, I shall retain them. Some authors, however, tend to use the label ‘Model-Theoretic Approach’, which I am inclined to find more accurate and applying to both of these branches.

Suppes is primarily concerned with arguing that set-theoretical axiomatisation of theories not only is a convenient means of representing theories, but is superior in various respects to standard formalisations (i.e. formalisation in first-order language with identity). Within the structuralist approach claims about the primary significance of set-theoretical structures in science, *vis-à-vis* the significance of formal language structures, are backed by worked out examples of set-theoretical axiomatisations, e.g.

classical particle mechanics, classical collision mechanics, relativistic collision mechanics, decision theory etc. (Balzer *et al.* 1987). In addition, within the structuralist program, attempts are made to elucidate the character of scientific progress and theory individuation. The Semantic approach takes from Suppes the model-theoretic view of theories and expands in this direction to maintain the claim that theories are families of models (i.e. mathematical structures). Differences among the two approaches exist, to name a few: (a) The Structuralist approach maintains a theoretical/non-theoretical distinction, hence positions itself closer to the Received View, whereas the Semantic approach does not. (b) In the Structuralist approach a canonical language of theory formulation (*viz.* set theory) is prescribed, whereas the Semantic approach allows for the language formulation to be dictated by the subject matter of the particular theory. (c) The structuralist approach treats theories as dynamic organisms and allows for theory individuation on the basis of a theoretical core and a core of intended applications, whereas the Semantic approach treats theories in a rather 'static' way where theory development is viewed as a sequence of independent theories.

Strictly speaking however, both approaches are structuralist, in the sense that both emphasize the structural features as opposed to the physical content of scientific theories. Hence, for the purposes of my arguments I do not think that much exists as to significantly distinguish the two approaches, other than the mathematical and idiomatic preferences of their proponents. In my work, I shall be focusing on the Semantic View, which I will henceforth refer to in abbreviation SV.

Before I enter into an analysis, elucidation and eventually critique of the SV of theories as it appears in the works of Van Fraassen, Suppe and Giere, I shall begin with a brief overview of the work of Suppes. I do this on the one hand with a historical conscience, but on the other because the proponents of the SV retained many of his views and adopted others with only minor alterations. Hence, comprehending the main elements of his work can assist us in understanding the SV.

In the 1950's and 1960's Patrick Suppes²³ was one of the major denouncers of the attempts by the Logical Positivists to characterise theories as formal (first-order) calculi supplemented by a set of correspondence rules. His objections to the Received View led him, on the one hand, to indicate that in scientific practice the theory/experiment relation is more sophisticated than what is implicit in the RV. Theories (or more accurately theoretical predictions) are not confronted with 'raw' experimental data but with what he calls 'models of data'. On the other hand, his disinclination to the RV led him to propose that theories should be viewed as collections of models of the theory. The models are possible realisations (in the Tarskian sense) that satisfy all valid statements of the theory, and these models are entities of the appropriate set-theoretical structure. Both of these insights/contributions have been operative in the conception and shaping of the SV, hence they call for elucidation.

3.1.1 Set-Theoretical Axiomatisation of Scientific Theories

Suppes' attempt to move towards set-theoretical axiomatisations of theories rested mainly on the contention that standard formalisations of scientific theories are a far too simple sketch. Firstly, no substantive example of a scientific theory is worked out in a formal calculus, and secondly its '...very sketchiness makes it possible to omit both important properties of theories and significant distinctions that may be introduced between different theories' [Suppes 1967, p57].

The simplicity of set-theoretical axiomatisations can be illustrated by looking at examples of such. What follows is a set-theoretical axiomatisation of Classical Particle Mechanics (CPM), which in essence is a definition of a CPM system as indicated by the intended physical interpretation.²⁴

²³ Suppes 1957, 1961, 1962, 1967, 1967a, 1969.

²⁴ See for instance, Suppes 1957 where he develops a CPM set-theoretical axiomatisation. I supply the intended physical interpretation only for purposes of clarity.

Statement of Kinematical Axioms:

Axiom 1: The set P is finite and non-empty.

Axiom 2: The set T is an interval of the real number line.

Axiom 3: For $p \in P$, the vector $s_p(t)$ is twice differentiable on T .

Intended Physical Interpretation of Axioms:

P is the set of particles. Every $p \in P$ represents a particle (corpuscle).

Every $t \in T$ represents an instant of time.

$s_p(t)$ represents the position of particle p at time t , in 3-dimensional Euclidean space.

Statement of Dynamical Axioms:

Axiom 4: For $p \in P$, $m(p)$ is a positive real number.

Axiom 5: For $p, q \in P$, and $t \in T$,

$$f(p, q, t) = -f(q, p, t)$$

Axiom 6: For $p, q \in P$ and $t \in T$,

$$s_p(t) \times f(p, q, t) = -s_q(t) \times f(q, p, t)$$

Axiom 7: For $p \in P$ and $t \in T$,

$$m(p)\ddot{s}_p(t) = \sum_{q \in P} f(p, q, t) + g(p, t)$$

$m(p)$ is the numerical value of the mass of particle p .

Newton's 3rd law. $f(p, q, t)$ is the internal force (of a 2-particle system) that q exerts on p at t .

The direction of the force between p and q is along the axis joining their positions.

This is Newton's 2nd Law of motion, where \ddot{s} designates the 2nd time derivative, and g is the external force on p .

All the above predicates are defined in terms of notions of set theory, hence the name *set-theoretical* predicates. The structure $\wp = \langle P, T, s, m, f, g \rangle$ is then a system of a CPM if and only if it satisfies the above axioms, which are to be recognised as components of the definition of a CPM system. Such a system is obviously constituted by a limited class of primitive concepts, but consequences are entailed. The theorems of classical particle mechanics can be proved and derivative concepts can be defined. Such a structure (i.e. anything satisfying the above definition) is what logicians would label a (semantic) model of the theory, or more accurately a class of models. The convenience of using set theory to axiomatise scientific theories stems from the fact that, in addition to general set theory literally all of classical mathematics can be utilised in the mathematical framework within which to operate.

An obvious objection is that a standard formalisation can be used to express the axioms and theorems of the theory and subsequently define the class of semantic

models metamathematically, as the class of structures that satisfy the theorems of CPM. But what Suppes is proposing is that such a procedure is unnecessarily complex and tedious, and that the class of intended models can be singled out without any reference to syntax. Set-theoretical axiomatisations are hence to be viewed as providing the procedure by which the class of intended models of the theory is uniquely determined. In other words the means by which to directly define the class of models. Moreover, the separation of the set-theoretical characterisation of structures from the (syntactical) axioms of the theory allows for the introduction and use of axiom-free notions about models, such as isomorphism of structure.

One could ask, presuming that scientific theories can be expressed in, or have a description in set theoretical terms, does this justify the claim that they should be *identified* with set-theoretical structures? This question can be considered as an offspring of a more general one: just because scientific theories exhibit several structural features, and thus can be described by a mathematical structure, or satisfy a mathematical structure, does this justify the claim that they can be identified with the structure? I think that to address such questions one must look into the structural descriptions of theories and attempt a comparison with actual scientific theorising. Later on I shall be arguing among other things, in the context of the SV, that structural representations are only rational reconstructions of theories (in the sense described in Chapter 2), despite the insistence of some of its proponents to the contrary. That is to say, structural representations provide one way by which to explicate the finished product, i.e. the theory, but do not provide an adequate description of actual scientific theorising. For now, I will just claim that Suppes seems to have intended set-theoretical axiomatisations as rational reconstructions of scientific theories and not as accurate descriptions of actual scientific theorising. Unlike its predecessor (the RV), the basis and mode of reconstruction are not ‘statements’ (i.e. linguistic entities) but structures (i.e. extralinguistic entities). *Prima facie*, the advancement this mode of axiomatisation makes over the RV is that it is less laborious and less awkward. But this is not all Suppes points to.

3.1.2 Models of Data

Models of data, according to Suppes, are possible realisations of the experimental data. Just as there exist possible realisations of a theory (i.e. models of a theory), there also exist possible realisations of the experimental data. It is to models of data that models of the theory are contrasted. In other words, the theory/experiment relation is a mapping of structure. The RV would have it that the theoretical predictions have a 'direct analogue' in the observation statements. This view however, is, according to Suppes, a distorting simplification. To substantiate these claims, Suppes points to the fact that theories are loaded with theoretical concepts that have no direct experimental analogues. Furthermore, the confirming experimental data are discrete and finitistic in character, in contrast to the models of the theory that, by and large, contain continuous functions or infinite sequences. To adequately establish the link between theory and experiment various steps are involved.²⁵ Models of data are defined in terms of possible realisations of the experimental data, which can be thought of as evaluations of experimental evidence. That is, by various processes that involve the experimental design and the theories of experiment (experimental parameters, auxiliary theories etc.), we induce a transliteration of the raw data into a 'language' that bears a less indirect relation to the models of the theory. In order for this to be achieved, as Suppe points out in his own contribution to the theory of 'models of data', firstly experiments must be carried out in controlled and isolated circumstances. Secondly, various influencing factors that the theory does not account for, but are known to influence the experimental data, must be accommodated by an appropriate conversion of the data into canonical form.²⁶ This transliteration results in a set-theoretical structure, one that reflects the experimental data after several elements have been taken into account, e.g. experimental design and procedures, *ceteris paribus* conditions that are assumed to hold, the theories of experiment and auxiliary theories etc. Accordingly,

²⁵ See Suppes 1962. Admittedly, Suppes' use of the example from Learning Theory indicates the various complexities involved in this process. Without intending to do any injustice to his analysis, I here present a much-simplified picture that suffices for the purposes of this work.

²⁶ See Suppe 1974, pp102-109, and 1989 chapter 4, where he expands on Suppes' analysis.

the finished product that Suppes dubbed ‘models of data’ are structures expressed by set-theoretical predicates that bear a direct link to the models of the theory. Suppes’ picture of science as an enterprise of theory-construction and empirical testing of theories involves establishing this ‘hierarchy of models’, roughly consisting of the general categories of models of the theory and models of the data. Furthermore, since the theory/experiment relation consists of a comparison of mathematical structures, it allows him to invoke the mathematical notion of *isomorphism of structure* to identify the link between theory and experiment. Hence, Suppes can be read as urging the thesis that defining the structures or models of the theory and checking for isomorphism with models of data, is a rational reconstruction that does more justice to actual science than the RV does.

It is noteworthy that the backbone of a structuralist account of theories is the sharp distinction between models of theory and models of data. The traditional syntactic account of the relation between theory and evidence, which can be captured by the following simple schema: $T \wedge A \rightarrow E$ (where, T stands for theory, A for auxiliaries, E for empirical evidence), is rejected. In its place the following schema is erected: $A \wedge E \mapsto M_D$, $M_T \in S$, and $M_D \approx M_T$. Where, M_D stands for model of data, M_T for model of theory, S for the theoretical structure, \mapsto for ‘...used in the construction of...’, \in for the relation of membership, and \approx for the relation of isomorphism. In short, this distinction implies that pure ingredients of the theory are used to construct the models of the theory, and everything else used in the attempt to relate theory to experimental evidence enters in the construction of the data models.

The proponents of the SV adopt the above distinction with reverence. Where the SV does digress from Suppes’ theory is in the adoption of a canonical language (such as set theory) in which the mathematical structure is presented. The proponents of the SV share with Suppes the claim that we start from an unstructured set of measurements, and in the process we give them some structural form, but according to the proponents of the SV the latter tends to be dictated by the language of the theory and the auxiliaries employed. It is evident that Suppes’ use of set theory gives rise to a picture of scientific theories which is totally disengaged from problems

concerning the language of theories, such as the problems of counterfactuals and modalities. In fact the use of set theory tends to emphasise an extensional character to scientific theories, where the modal structure is concealed. The Semantic View on the other hand, avoids this problem for in the latter the presentation of the structure is chosen as to let ‘...the appropriate language be dictated by the specific scientific subject under investigation’ [Giere 1985, p76].

In the last decade or so, we have witnessed a significant amount of philosophical work that explores the role of scientific models in the theoretical representation of phenomena. This newly born tradition focuses on questions of how models represent real physical systems and how they relate to theory. It is important from the outset to distinguish this philosophical tradition from structuralist (model-theoretic) views of scientific theories, and indeed the SV. Both philosophical accounts maintain that models are devices of scientific representation. This common feature, however, is not sufficient to render the two accounts compatible with each other. The model-theoretic approaches differentiate themselves from the logical positivist program on the grounds that language plays essentially no role in theoretical representation. Language, whether the language of set-theory or that dictated by the subject matter of a particular science, is given the expressive role of presenting the classes of models (i.e. mathematical structures). Theoretical representation is thus reduced to some form of structural mapping. Implicit in model-theoretic approaches is therefore the idea that science only gives the structure, as opposed to the physical content, of the phenomena. This idea can be put forward in the following schematic way: once the structure of the theory is defined we make available the class of intended models for modelling the particular physical domain. This is roughly what scientific theorising consists of in the model-theoretic view. The view that models are scientific representation devices is not however a monopoly of the model-theoretic approaches. The spirit of my approach in the remaining of this work is that scientific theories are collections of interpreted statements to be understood literally. Although language is one of the means of representing something extralinguistic, i.e. the world, I do not understand theories as representing the world in any direct sense. In fact, an intermediary medium of representation is necessary in the majority of cases: the scientific model. It is with this disposition that I intend to criticise the SV and not for

restoring arguments in favour of Logical Positivism. But before I enter into the various dimensions of my argument I want to scrutinise the SV in an attempt to understand it, to understand its use of the notion of ‘model’, and to extract its most important implications.

3.2 The Semantic View of Scientific Theories

The major proponents of the SV of scientific theories are van Fraassen, Suppe, Giere, Lloyd, da Costa and French.²⁷ In my treatise of the SV I will rely primarily on van Fraassen’s ‘state-space’ approach and Suppe’s ‘relational-systems’ approach. The differences among the two are inconspicuous but significant in a context that ultimately will become clear. Despite the fact that in the recent literature the state-space approach has become the predominant representative of the SV because of its simplicity, I believe there is a sense in which the two versions complement each other -particularly on the fact that the relational-systems approach can accommodate both mathematical and non-mathematical theories in contrast to the state-space approach. Moreover, Suppe explicitly treats elements of scientific theorising (such as, abstraction and idealisation) that require analysis without stripping them from his own philosophical and methodological considerations. I will argue that these features subsequently render his approach -or more precisely his understanding of the state-space and the theoretical representation of phenomena- the most defensible thesis for the SV. Apart from this, it is important to note that van Fraassen presents us with a fragmented sketch of the SV. It is fragmented because different features are treated in different writings at different times, and often different terminology is employed. It is

²⁷ Van Fraassen 1967, 1969, 1970, 1971, 1972, 1980, 1985, 1987, 1989, 1991, 1997; Suppe 1972, 1972a, 1973 1974, 1977, 1979, 1989, 1998; Giere 1985, 1988, 1988, 1991, 1999; Lloyd 1988; da Costa and French 1990. Lloyd applies the Semantic View to evolutionary theory. Da Costa and French and their collaborators develop a version of the Semantic View that uses partial structures to claim the unity of models and theory, i.e. models of the theory are subsumed under a unifying theory structure by sharing only parts of their own structure with the theory. Both of these undertakings require attention, which I do not claim to have given them. It is therefore likely that my arguments do not affect their views.

sketchy because in very few of his writings do we find a detailed analysis of the concepts he employs. Subsequently it is left to the reader to put all the pieces together, an undertaking that may lead to inconsistencies, as well as, injustices to van Fraassen's position. By contrast, Suppe provides us with a complete and to a large degree precise exposition of the SV, thus such misapprehensions can be avoided.

We have seen in Chapter 2, that none of the arguments against the Received View convincingly lead to repudiating the view that theories can be rationally reconstructed as formal calculi augmented by a set of correspondence rules. The collection of criticisms rather acted as a catalyst in the recognition that the RV is faced with possibly insurmountable problems. The model-theoretic approaches are hence constructed as to avoid those problems faced by the RV. There is however, a further motivation captured by Suppe in the following argument. Suppe charges the RV that it confounds the formulation of a theory with the theory itself. He claims that this is not justifiable because a particular theory may be expressed in more than one language (linguistic formalism).²⁸ In his words, 'theories admit of a number of alternative linguistic formulations', for example the Lagrangian or Hamiltonian formalisms of CPM and the wave or matrix mechanics of the Quantum theory. So he invites us to distinguish between the theory, an extra-linguistic entity, and its linguistic formulations. By making a distinction between the theory and the theory formulation (justifiably or not), the semantic approach is thus not faced with problems associated with an observation/theory distinction or an analytic/synthetic distinction in vocabulary terms, because it identifies theories with extra-linguistic entities.

The structure of my exposition is the following. I will first analyse the structural account of scientific theories as proposed by van Fraassen. In the process I will examine the physical interpretation and theory/experiment relation as accounted for by the state-space approach. I will then proceed to elaborate on Suppe's approach, analysing its structural account, physical interpretation, and theory/experiment relation, with primary focus on the features that distinguish it from that of van

²⁸ A claim shared by van Fraassen and other proponents of the Semantic View.

Fraassen. In addition to understanding the SV, the focus in this Chapter is primarily to bring to the surface those of its features that are either untenable or distance it from actual scientific practices. In doing so, the strategies by which to criticise the SV can be illuminated. Thus the basis can be built for expanding on the objections to be sketched at the end of this chapter. Criticisms of the SV will follow in the next Chapters.

3.2.1 Van Fraassen's State-Space Version

Van Fraassen is primarily concerned with the internal structure of scientific theories. Only in the more recent of his writings does he come to endorse Giere's work on how theoretical models, and more generally theories, are applied. I will begin by outlining the structural elements he attributes to theories and then proceed to construct the initial and the more recent of his views for applying them.²⁹

3.2.1.1 The Structural Elements of Scientific Theories

Van Fraassen's view of scientific theories is that they consist of three features, (1) a state-space that represents the states of a physical system, (2) a set of elementary statements about measurable physical magnitudes, and (3) a mapping (satisfaction function) of the elementary statements onto the state-space. The objects of concern of scientific theories are physical systems. Typically, mathematical models represent physical systems that can generally be conceived as admitting of a certain set of states. *State-spaces* are the mathematical spaces the elements of which can be used to

²⁹ Van Fraassen has through the years changed his mind about certain characteristics of the SV. This has been the consequence of his own rethinking of several of the elements of the approach, but also because of other people's contributions. Although I am here presenting the major elements of the SV, we must not lose sight of the fact that it is still a 'paradigm' in development. As such I do not think the most defensible thesis for it has yet been constructed, hence presenting 'older' as well as 'newer' versions of it, and various people's contributions to it are necessary ingredients to a comprehensive understanding.

represent the states of physical systems. It is a general term used to refer to what for example physicists would label as phase space in classical mechanics or Hilbert space in quantum mechanics. A simple example of a state-space would be that of an n -particle system. In CPM, the state of each particle at a given time is specified by its position $q=(q_x, q_y, q_z)$ and momentum $p=(p_x, p_y, p_z)$ vectors. Hence the state-space of an n -particle system would be a Euclidean $6n$ -dimensional space, whose points are the $6n$ -tuples of real numbers $\langle q_{1x}, q_{1y}, q_{1z}, \dots, q_{nx}, q_{ny}, q_{nz}, \dots, p_{1x}, p_{1y}, p_{1z}, \dots, p_{nx}, p_{ny}, p_{nz} \rangle$. More generally a state-space is the collection of mathematical entities such as, vectors, functions, or numbers, which is used to specify the set of states that a particular physical system could allow, in different words it prescribes all possible instantiations of a system.

State-spaces unite clusters of models of a theory, and they can be used to single out the class of intended models just as set-theoretical predicates would for Suppes' analysis. The presentation of a theory consists of a description of *a class of state-space types*. The way to understand this can be demonstrated by means of an example. CPM systems are n -particle systems represented by a state-space which is a $6n$ -tuple of real numbers. This would mean that a k -particle system and an m -particle system, where $k \neq m$, will need a different state description of different dimension. In order for this to be accommodated into the state-space approach van Fraassen identifies the theory with the class of state-space types. Where, each state-space type is the set of states s_n associated with the n -particle system. For example, s_k is the set of states of a k -particle system, s_m of an m -particle system and so forth. The class of such types for CPM is the class of an infinite number of state-spaces, each associated with an n -particle system of different dimension. More generally, as van Fraassen puts it '[w]hen certain parameters are left unspecified in the description of a structure, it would be more accurate to say ...that we described a structure-type.' [1980, p44] The Bohr model of the atom, for example, does not refer to a single structure, but to a class of structure types that share some general characteristics. Once the necessary characteristics are specified it gives rise to a structure for the hydrogen atom, a structure for the helium atom, and so forth. Another reason for identifying the theory with a class of state-space types is that, in theories such as the

Special Theory of Relativity the frame of reference determines a particular configuration imposed on phase space (given by the Lorentz group of transformations). We may thus construe a state-space type either as consisting of the set of states of a system of particular dimension for all frames of reference of that system; or as the set of states for a particular dimension and a particular frame of reference.³⁰

In addition to the state-space, the theory (according to Van Fraassen) characterises physical systems by the use of a class of *measurable physical magnitudes*. Such physical magnitudes are represented in classical mechanics by real valued functions defined on the state-space, whereas in quantum mechanics they are represented by Hermitian operators. A set of *elementary statements* about the system is utilised. Each elementary statement formulates a proposition that asserts a particular value (or range of values, Borel sets -a view van Fraassen adopts in the more recent of his writings) for a particular physical magnitude at a particular time.³¹ Thus, for instance, an elementary statement $U=U(m, b, t)$ expresses the proposition that: the physical

³⁰ It should be pointed out that, a non-equivalent way to the above is to identify the theory with the *union* set of s_i 's, in which case there will be one state-space representing all types of all dimensions. That the two ways are not equivalent is explained in Tarski and Vaught 1957. Couched in Tarski's language of relational systems (we could roughly view relational systems as state-spaces) it can briefly be put as follows: a union of relational systems (state-spaces) is an *arithmetical extension* of its components if it is a *directed class* of relational systems. We (informally) define \wp to be an arithmetical extension of one of its subsystems \mathfrak{R} if, \wp is an extension of \mathfrak{R} , and whenever every element of \mathfrak{R} satisfies any formula of a first-order language in \mathfrak{R} , they also satisfy it in \wp , and conversely. Now, let K be a *directed class* of systems if any two systems in K have a common extension which is also in K . Tarski and Vaught explain that if, K is a directed class then the *union class* of K is also an extension of each system belonging to K . Now since the class of state-spaces described above is non-directed, and given that two mathematical systems are elementarily equivalent if every sentence which is true in one is also true in the other, it follows that the two ways are not equivalent. I believe that van Fraassen, as well as all the proponents of the SV, chooses to identify the structure with a class of state-space types merely on the grounds that it is closer to the practices of scientists.

³¹ In quantum mechanics of course the situation is different, but it can easily be accommodated if elementary propositions are understood as asserting the probabilities of the eigenvalues of the operators.

magnitude m (such as the mass of a particle) has value b at time t . The truth or falsity of the formulated propositions depends on the state of the system in concern. Hence a relation between the state-space and the set of elementary statements expresses a relation between the states of the space and the values of physical magnitudes of a system. This relation is established by means of a *satisfaction function* $h(U)$, which is a mapping of each elementary statement onto a region of the state-space that satisfies the proposition expressed, i.e. U is true of the system if and only if $h(U)$ represents the actual state of the system.

As an example consider the three-dimensional motion of a classical particle. The particle is in a state $\langle m, \mathbf{r}, \mathbf{v} \rangle$, if it has mass m , position $\mathbf{r} = (x_1, x_2, x_3)$, and velocity $\mathbf{v} = (v_1, v_2, v_3)$ at a particular time t . If U expresses the proposition that the kinetic energy of the particle at time t equals E , then $h(U) = \{ \langle m, \mathbf{r}, \mathbf{v} \rangle : \frac{1}{2}m\mathbf{v}^2 = E \}$. Hence, $h(U)$ can be understood as depicting the set of states that satisfy U . So far for the sake of simplicity of exposition, I have ignored time dependence. To be more accurate however, we must note that the factor of time can be introduced into the SV by specifying how the state of the system evolves. Van Fraassen stresses that ‘...sometimes ‘state’ is used in such a way that a system (though undisturbed) has different states at different times, and sometimes such that a system remains in the same state unless it is subject to interaction’ [van Fraassen 1970, p329]. He suggests that in the first case, the satisfaction function is viewed as time-independent but the location of the system in state-space is viewed as evolving in time (applicable to isolated systems). In the second case, the satisfaction function is viewed as time-dependent (applicable to interactive systems), in which case it would mean that the system remains in the same state unless it is subject to interaction. However, physical magnitudes could change even though the system remains in the same state.³²

How does all this fit together into van Fraassen’s picture of scientific theories? The state-space defines the mathematical models that constitute the theory. The satisfaction function maps the regions of the state-space to propositions about measurable physical magnitudes of the theory. Thus the link with empirical

³² See van Fraassen 1972, for examples of these roles of the time variable.

measurement is established. As van Fraassen admits (1970, p329), this may almost sound like an operationalist thesis. Yet the correspondence rules of the RV cannot capture all the constituents of this picture, since the elementary statements of the theory are confronted with experimental reports (in Suppes' sense of models of data). Herein lies, I believe, a departure from Suppes' set-theoretic approach. Models of data are not expressed as set-theoretical predicates but as sets of statements about measured quantities. These sets of statements are formulated in the appropriate language of the theory in question, and this is enabled by the fact that the structure of the state-space is defined in that same language.

Another aspect of van Fraassen's view is that the state-space provides the interpretation of the elementary statements. Moreover, the state-space together with this set of statements and the satisfaction function form a language associated with a given theory.³³ This is not a language within which the theory is formulated, but one '... in which statements about the *subject matter* of the theory can be formulated. Exploring the structure of the elementary language is one way of exploring what the theory says about the world.' [van Fraassen 1972, p312, the emphasis is mine.] Since all elementary statements are mapped onto the state-space, the mathematical structure of the space is induced onto the meaning relations among the predicates used to characterise a physical system. Certainly the laws of the theory (which should not be identified with the axioms or a subset of the axioms in a syntactic reconstruction of a theory) also induce restrictions. But as Mackinnon (1979, p522) points out, 'in any given formulation of a theory there is a clear distinction between statements which are true by virtue of meaning relations and statements which are true by virtue of laws.'³⁴ If for a certain kind of physical system X , the theory specifies a state-space H , a set E of elementary statements, and a satisfaction function h , then a *semi-interpreted language* comprises of the triple $L=\langle H, E, h \rangle$. A couple $M=\langle loc, X \rangle$ is a

³³ Sometimes van Fraassen refers to such a language as *semi-interpreted language* (1967 and 1970), and sometimes as *elementary language* (1972).

³⁴ The interested reader may inquire into van Fraassen 1967, 1969, 1970. Of particular interest is van Fraassen's attempt in addressing meaning relations among predicates as a substitute to explicating the

model for L , where loc is a function assigning a location to X in H . A semantic definition of truth can then be given in terms of the state-space: $U \in E$ is true in $M = \langle loc, X \rangle$ if and only if $loc(X) \in h(U)$. Through this definition we can discern two important features of the SV. First, the motto of the SV, ‘the class of models is defined by means of the state-space’, is to be understood as a direct definition of the class of mathematical structures (the set of objects).³⁵ Second, the satisfaction function provides the physical interpretation of theories, by mapping the experimental reports of physical systems onto regions of the state-space. These physical interpretations are implicitly specified and are presumably subject to change. The semi-interpreted language is thus partially interpreted, but not in a Carnapian sense. It is partially interpreted in the sense that elementary propositions are mapped onto the state-space. A full interpretation comes about by means of the location function that relates the physical system to the state-space.

These considerations that van Fraassen takes us through are essentially an attempt to assign to language a purely descriptive role. That is to say, the representation capacity is stripped away from language and shifted to the theory structure and the models of the theory. Language is given the purely expressive role of defining the structures that constitute the theory.

3.2.1.2 Applying the State-Space Approach

We can look at the Newtonian description of the solar system as a particular physical system by which to make sense of the SV.³⁶ We must note the fact that the operative notion in this system is that of relative motion. Hence for the sake of the argument we

notion of the *intent* of a predicate. An attempt that reveals part of the motivation which underlies the Semantic View.

³⁵ It is among the claims of the proponents of the SV that this is conspicuously different from the sense of ‘model’ as an interpretation that satisfies a set of statements (the axioms and theorems of a theory). An interpretation, that is, that matches a set of statements to a set of objects for which certain relations hold. This issue will be addressed in more detail in section 3.3.

³⁶ A system that van Fraassen himself chooses to use for his description, 1980, pp44-46.

can ignore Newton's notion of 'true motions', i.e. motions relative to absolute space. In describing the behaviour of the system, Newton chooses to speak of apparent motion, which is motion relative to the earth, i.e. the observer, and of relative motion of the planets. In general, one can speak of relative motion with respect to the earth, or the sun, or any inertial frame of reference. As van Fraassen points out, the relative motions of the planets '...form relational structures defined by measuring relative distances, time intervals, and angles of separation.' [1980, p45] He calls these relational structures *appearances*, which can be regarded as the equivalent of models of data. Within the mathematical model (i.e. an element of the state-space for CPM) of this physical system, '...we can define structures that are meant to be exact reflections of those appearances...' [*Ibid.*, p45]. These structures are what are known, in van Fraassen's jargon, as *empirical substructures*. Empirical substructures are defined, so to speak, by the satisfaction function of the theory. The latter singles out those regions of the state-space that satisfy those elementary statements, which formulate propositions about the motions of the planets. Van Fraassen's view is that, claims of theory representation of observable phenomena are claims about *isomorphism* between all actual appearances and the empirical substructures of some model of the theory. For instance in the case of the Newtonian solar system, the appearances (i.e. the observation reports about relative distances, time intervals and angles of separation) are contrasted against the appropriate parts of the state-space. Van Fraassen's view is that, if the theory is empirically adequate then the appearances are embedded in the models of the theory, or they are isomorphic to the empirical substructures of some model. Since the state-space is to be understood as a cluster of models of the theory, it includes many models in which the world is a Newtonian mechanical system. In fact the state-space includes (unites) all logically possible models, as the following 'completeness' dictum suggests: 'In one such model, nothing except the solar system exists at all; in another the fixed stars also exist, and in a third, the solar system exists and dolphins are its only rational inhabitants.' [van Fraassen 1987, p111; and 1989, p226]³⁷ Hence, if the theory is

³⁷ This view does not only imply that the state-space unites the models of the theory, but also that by virtue of its infinite class of models the theory includes the best model for the representation purpose at

empirically adequate we can presumably find a model of the theory in which we can specify empirical substructures that are isomorphic to the appearances or data model.

We can use van Fraassen's own encapsulation of his picture of scientific theories to recapitulate the above:

‘To present a theory is to specify a family of structures, its *models*; and secondly, to specify certain parts of those models (the *empirical substructures*) as candidates for the direct representation of observable phenomena. The structures which can be described in experimental and measurement reports we can call *appearances*: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model.’ [Van Fraassen 1980, p64]

I have made no allusion to the empirical adequacy requirement, vis-à-vis ‘truth requirements’, for theories because in my discussion the debate over scientific realism and anti-realism is of no particular relevance. In order to make sense, however, of the notion of ‘empirical substructures’ we have to look into two essential ingredients of van Fraassen's constructive empiricism. The first is that of the observable/nonobservable division regarding features of the models. Appearances are relational structures of measurements of observable aspects of the physical system, e.g. relative distances and velocities. Hence the empirical substructures are those parts of the model that are presumed to be isomorphic to the observable aspects of the physical system, i.e. the appearances. The second ingredient is that of modality in the models. Van Fraassen allows modalities only to exist within our models. For instance the model allows for any sequence of states for different initial conditions. This however is only a consequence of our model construction; it says nothing about physical modalities, which van Fraassen rejects and insists that beliefs of this sort are unwarranted: ‘To be an empiricist is to withhold belief in anything that goes beyond the actual, observable phenomena, and to recognise no objective modality in nature.’ [*Ibid.* p202] He contends that in our measurements we are only able to observe the actual occurrences. Van Fraassen's modal agnosticism is without doubt an interesting subject for philosophical inquiry, yet the point relevant to the present discussion is that the empirical adequacy requirement ‘...concerns actual phenomena: what does happen, and not what would happen under different circumstances’ [*Ibid.*, p60], as

hand, and furthermore that the theory provides all the instructions necessary for choosing that model. These implications of the SV will be explored and criticised in Chapters 4 and 5.

this marks one of the major differences between van Fraassen's and Suppe's approaches. This requirement of course has as corollary the fact that all appearances must be isomorphic to empirical substructures of the model. This, as Suppe points out [1989, p102], has another dimension. It would require that all physical systems in the domain of the theory occur in isolated circumstances and under idealised conditions.

To conclude the presentation of the state-space approach, we must make reference to a significant amendment/addition van Fraassen makes to his original conception of the nature of theories and their representation of phenomena. By adopting from the work of Giere, he identifies a theory with two elements, '...(a) the *theoretical definition*, which defines a certain class of systems; (b) a *theoretical hypothesis*, which asserts that certain (sorts of) real systems are among (or related in some way to) members of that class' [van Fraassen 1989, p222]. The theoretical definition evidently consists in the definition of the class of models, i.e. the state-space or the class of state-space types. The theoretical hypothesis is however a linguistic entity, it asserts that a certain model or class of models (the empirical substructures) is/are isomorphic to the appearances (or models of data).³⁸ In other words, theoretical hypotheses are asserted claims that an individual real system exhibits -all or possibly some of the features of- the structure of a model in the defined class. They could also be used to make general claims that for a certain class of real systems all members exhibit the structure.

³⁸ Giere suggests the relation of 'similarity in respects and degrees', rather than 'isomorphism', between models and phenomena, because he admits that isomorphism is too strong a demand (special emphasis must be added to the fact that Giere is not confining 'similarity' only to the observable aspects of the models). However, his notion of similarity has been criticised convincingly for its vagueness. One way to interpret Giere's use of the notion is (along the same lines as Suppe) to consider it as suggesting that models inherently involve abstractions and idealisations, in which case it would fall prey to the criticisms that follow in the subsequent Chapters.

3.2.1.3 Theoretical Representation by Means of the State-Space

In figure 3.1 I present a schematic of the view proposed by van Fraassen (as with every picture, of course, an element of abstraction is unavoidable). We can use this figure to recapitulate van Fraassen's approach. There is a class of real systems in the world for which a particular language (call it elementary language) may be used to characterise certain features of the former. These features are the particular relations that determine the scope of the theory, e.g. mechanical relations determine the scope of CPM. Certain parameters are hence developed in the language as to address the subject matter of each domain. The relation that exists between the use of language and the real systems (the world) is not addressed (one is led to assume that it is a subject for empirical investigation, rather than one of proper philosophical inquiry). The language only serves the purpose of defining a class of structure-types called the state-space types. This class of structures (in Section 3.3 it will be labelled 'families of theoretical models') is made available for modelling the domain of the theory.

The real systems are subjected to experimentation, where measurements of data involve not only experimental design and data recordings but also corrections of raw data accounting for *ceteris paribus* conditions, experimental calibrations etc. The results are models of the data or appearances, which are structures defined in the language of the theory (the dotted lines in the diagram indicate the use of the language of the theory in constructing models of data). The SV, in van Fraassen's version, claims that a certain relation exists between the models of the data and an empirical substructure of the state-space. This relation is one of embedding or isomorphism, and it is expressed by means of a linguistic entity called a theoretical hypothesis.

Thus we may conclude that theoretical representation involves the definition of the theory structure. Defining the theory-structure yields an indefinite number of models, which become available for representing the phenomena in the theory's scope. The application of a theory consists in determining which model of the theory is best

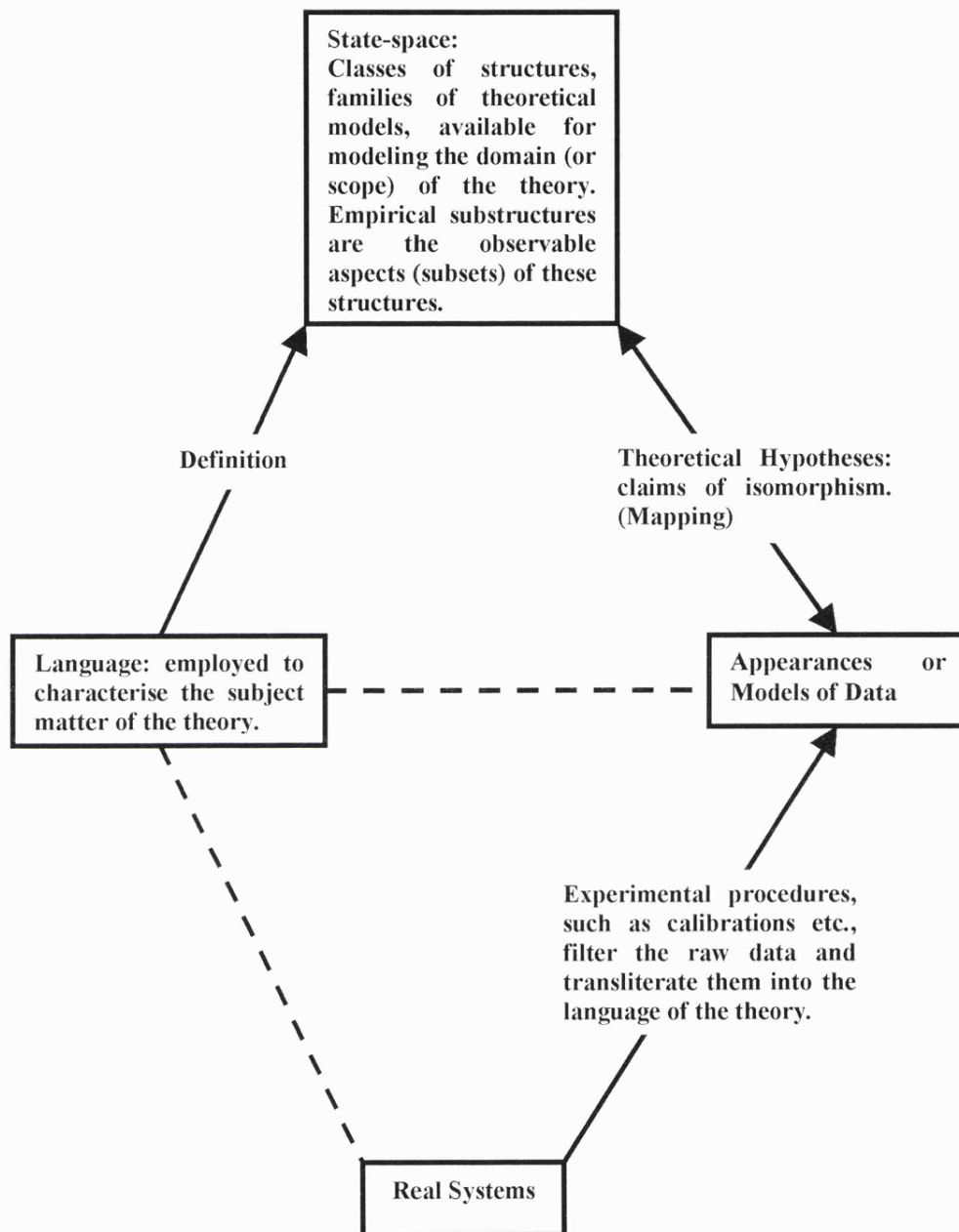


Figure 3.1

suited for representing a particular physical system. Once the model is chosen, the representation relation it bears to the corresponding data model is of structural nature, i.e. isomorphism or embedding. More precisely, the data model is isomorphic to an empirical substructure embedded in the model. This view of theory-application, and subsequently of theoretical representation, can be disputed on two fronts. Firstly we can argue intrinsically, i.e. within the spirit of the SV (as we shall see, Suppe's dispute with van Fraassen's version is along this line), that isomorphism of structure is rarely if ever detected in actual scientific practices. But secondly, and more importantly we can argue extrinsically, i.e. outside of the spirit of the SV and consequently against the SV, that a number of unsatisfactory and often unjustifiable and highly disputable assumptions underlie this view of theoretical representation: (1) That in actual scientific theorising, there can be a sharp distinction between models of theory and models of data. (2) That in actual scientific theorising when we define a theory-structure we immediately lay down an indefinite number of models that are available for modelling the theory's domain. (3) That the actual scientific models used in theoretical representation approximate, in one form or another, one or some models of the theory, or that they are the pragmatic counterparts of corresponding models of the theory. (4) That the methods and processes of construction of actual representation models are irrelevant to how these models relate to theory and to how they function in scientific inquiry. (5) That modelling in science, i.e. the construction of representation models, is done by having a model of the theory as a starting point.

We shall see in the next section (3.2.2) how Suppe gives an alternative to van Fraassen's representation relation. However, we shall also see that his account also relies on the above assumptions. Indeed, I fail to see how any rational reconstruction of scientific theories that uses mathematical structures could avoid these assumptions.

3.2.2 Suppe's Relational-Systems Version

Suppe's analysis of his version of the SV is much more elaborate than any other. There is however one drawback that makes its assimilation fatiguing, the fact that he uses an uncommon and idiosyncratic terminology. He does this because he wants to

establish a general terminology that can be used with reference to all theories, mathematical and non-mathematical. This terminology will be explained as I proceed with my explication of his version of the SV, by supplying my own examples from classical mechanics. My own examples, whose objective is to make my presentation more intelligible, are carefully chosen so that Suppe's view is not distorted.

As pointed out earlier, Suppe attempts to construct a version of the SV that can also accommodate non-mathematical theories. In doing so he identifies the theory with structures that are more general than the state-space, for which state-spaces are canonical models of the former. He claims three reasons for doing so. Firstly, as mentioned, he wants to also accommodate in his approach non-mathematical theories. Secondly, if theories are identified with configurations of numbers imposed on phase spaces, or more generally state-spaces, then theories like CPM would be comprised of an infinite number of theories, each representing an n -body system of a different dimension. We have seen in section 3.2.1.1 however, that van Fraassen does not identify the theory with a state-space but rather with a class of state-space types. He does so precisely as to avoid this problem. Therefore, I believe Suppe's second reason can be dismissed. Thirdly, in some theories, as for example the Special Theory of Relativity, each frame of reference may be construed as determining a different class of configurations imposed on phase spaces. But the way these phase spaces are interconnected, namely by way of the Lorentz group of transformations, is a central part of the theory. Hence it would be a mistake to view such configurations imposed on phase spaces as the theory. But as Suppe himself recognises, this problem can also be avoided by considering the Lorentz transformations as laws of coexistence, and the configurations of all frames of reference as being imposed on the same phase space.³⁹ Thus, it is my opinion that the only sustainable argument for Suppe's relational-systems approach as opposed to the state-space, is that it is more general and allows the accommodation of non-mathematical theories into the framework of the SV. This is not an argument, however, to be looked upon with contempt, for

³⁹ See Suppe 1974a, pp227-228, and Suppe 1989, pp103-106, for an analysis of these points.

Suppe sanctions the SV as a respectable contender for understanding the nature of all scientific theories and not just those given in terms of a mathematical formulation.⁴⁰

3.2.2.1 The Relational-Systems Approach

According to Suppe, scientific theories are extralinguistic structures that qualify as semantic models of their linguistic formulations. Theories characterise particular classes of phenomena known as the *intended scope of the theory*. However phenomena are not characterised in their full complexity, but certain factors or parameters (not necessarily measurable) are abstracted from them and used in the description of *physical systems*.⁴¹ Physical systems are abstract in the sense that they utilise only the abstracted parameters to characterise phenomena. These parameters are the *defining parameters of the physical systems* since they are used to wholly describe their behaviour. For example, the intended scope of CPM is the class of all mechanical phenomena of interacting bodies, and its defining parameters are the position and momentum vectors. Physical systems are abstract replicas of phenomena, in the sense that they are what the phenomena would have been if their behaviour depended only on the selected parameters. For example, a real pendulum is subject to a large number of influences, among them disturbances due to the medium in which it oscillates. CMP would not describe the behaviour of a real pendulum, but of a respective abstract physical system that would be assumed to operate, for instance, in vacuum.

⁴⁰ I will be using the same terminology and notation as Suppe 1989, which is his most elaborate work and on which I rely heavily. The reader should be cautioned however, that I analyse Suppe's general version of the SV by employing CPM as an example (i.e. a mathematical theory). This should not cause any ambiguity as long as the feature of generality is recognised in his analysis, and it is distinguished from the state-space approach. Suppe himself emphatically attempts to establish the connection of mathematical theories with his own analysis to stress this generality. See for instance his 1989, pp104-106, where he employs a variant terminology of exposition for mathematical theories. For purposes of simplicity, I have chosen to ignore the latter terminology.

⁴¹ For the remaining of this section, 'physical system' will be used to denote this particular notion developed by Suppe, which becomes evident in the discussion that follows.

The defining parameters of each physical system are the *basic parameters* of its associated theory, hence all physical systems are described in the language of the theory. The values of the defining parameters are *physical quantities*.⁴² A possible *state* of a physical system is a set of simultaneous values of the defining parameters. Physical systems are in one possible state at any given time, though this particular state may change over time. The *behaviour* of a physical system is the time evolution of its states. Suppe invites us to view the unique sequence of states a physical system assumes over time as its ‘history’. A complete characterisation of a physical system would involve the specification of the possible states it can assume in conjunction with its history.

The physical system (in Suppe’s terminology) is the medium of theoretical description, but since it is constructed from abstracted parameters, the description is counterfactual. The physical system, and subsequently the theory, describes what the phenomena *would have been* had their behaviour been subject only to influences from the abstracted parameters. The behaviour of actual phenomena may of course be subject to other unselected parameters, for which the theory does not account. Within the theory’s intended scope, each physical system *S* will correspond to a *causally possible phenomenon P*, e.g. the pendulum physical system which is described by ignoring a large number of influencing factors corresponds to an actual pendulum in the world. This correspondence is counterfactual, *S* is what *P* would have been if *P* were influenced only by the selected parameters and were the *idealised conditions* imposed by the theory met.

We may define the class of physical systems that correspond to causally possible phenomena within the theory’s intended scope, in the above manner, to be the class of *causally possible physical systems*. One of the jobs of a scientific theory is ‘...to

⁴² We can think of the physical quantities as the equivalent of van Fraassen’s measurable physical magnitudes, although I stress again that for Suppe these may be qualitative in nature and hence not measurable. Suppe himself gives the example of ‘colour’ fitting the prerequisite of being a defining parameter, in which case the physical quantities would be ‘differentiated colours’. Another example of a physical quantity would be probability distribution functions.

exactly circumscribe the class of causally possible physical systems' [Suppe 1989, p84]. This is done by determining the class of *theory-induced physical systems* (a notion that is explained below). And the truth or falsity of a theory is determined by whether the two classes are identical or not. If the class of causally possible physical systems is identical to the class of theory-induced physical systems then the theory is true, and false otherwise. Suppe contends that theories have two features, *viz.* they are propounded extralinguistic structures and they consist of a class of theory-induced physical systems. He suggests that these two features that can most easily be accommodated by analysing theories as relational systems (in a Tarskian sense) that consist of '...a domain containing all (logically) possible states of all (logically) possible physical systems...' [*Ibid.*, p84] together with the laws of the theory defined over that domain.⁴³ The laws of the theory indicate which states are *physically possible* and which sequences of states the physical system can assume. Therefore, the laws of the theory determine the relations of the theory, and consequently eliminate some logically possible states from qualifying as candidates for the behaviour of physical systems.

We are thus faced with the following picture. The extralinguistic structure consists of the domain of all logically possible states. The theory however also consists of certain attributes defined over the domain. These attributes are the laws of the theory, such as the general categories of *laws of succession*, *coexistence*, and *interaction* holding for either deterministic or statistical cases. For example, if the laws of the theory are laws of succession then the attributes will be *relations* of succession, if they are laws of coexistence then the attributes will be *equivalence relations*, and so forth. The attributes of the theory have two functions. Firstly, they indicate the sub-domain of *physically possible* states in the domain of the logically possible states. Secondly, (together with initial conditions) they indicate the sequences of states a physical system can assume. In conjunction these two features of the attributes of the theory

⁴³ It may, *prima facie*, seem that van Fraassen's equivalent to this is the state-space. State-spaces however, as mentioned earlier, can be viewed as particular instances (or canonical iconic models) of Suppe's relational structures, if unique sets of *n*-tuples of numbers are assigned to each and every state.

determine the class of theory-induced physical systems, which we can read as the class of theoretically possible physical systems determined by the laws of the theory.

As an example to help visualise Suppe's analysis, consider the following. The linear harmonic oscillator is a theory-induced physical system, because it is the result of Newton's 2nd law, which relates the position vector as given by a particular force function to its second derivative. It is at a level of abstraction (i.e. only the potential energy function, $V = \frac{1}{2}kx^2$, is specified) that bears a very distant resemblance to actual phenomena. But the key feature of systems like the linear harmonic oscillator is that they consist of mere definitions of a mathematical function (e.g. the potential energy). Given the constraints imposed upon them by the laws of the theory, functions such as this give rise to what Suppe calls theory-induced physical systems. Theory-induced physical systems do not directly relate to the world, but they 'circumscribe' causally possible physical systems. In other words, the linear harmonic oscillator is an abstract mathematical structure that nevertheless circumscribes other more 'concrete' forms of oscillators, such as the torsion pendulum or the mass-spring system. Both of the latter can be regarded as causally possible physical systems that characterise causally possible phenomena in the aforementioned counterfactual way.

It is worth looking at what takes place here. The linear harmonic oscillator, a 'mathematical instrument', has the following equation of motion: $\ddot{x} + (k/m)x = 0$, which is the result of applying Newton's 2nd law to the foregoing potential energy function. The mathematical model itself fixes the interpretation of the mathematical constituents of this equation: periodic oscillations are assumed to take place with respect to time, x is the displacement of an oscillating mass-point, k and m are constant coefficients that may be replaced by others. The torsion pendulum on the other hand, although it is also a 'mathematical instrument', resembles in various respects an actual causal phenomenon: an elastic rod connected to a massive object, e.g. a disc, whose normal to the tangent oscillates about an equilibrium position. Its equation of motion is $\ddot{\theta} + (K/I)\theta = 0$ where, θ is the angle of twist, K is the torsion constant and I is the moment of inertia. The torsion pendulum equation is identical in mathematical form (or isomorphic in structure) to the linear harmonic oscillator

equation, therefore angular oscillations must be harmonic. In identifying a theory-induced with a causally possible physical system, we unavoidably exercise aspects of physical interpretation, in particular, that aspect which Giere has dubbed ‘identification’ of mathematical terms.⁴⁴ For instance, in the torsion pendulum we transform the language of ‘the displacement of an oscillating mass-point...’ to that of an ‘object of specific geometric form that undergoes angular oscillations about a specified axis under abstract and idealised circumstances’. The behaviour of the linear harmonic oscillator is carried over to the torsion pendulum, but the affixed feature of geometric form reduces it from mathematical abstraction to a level that can reasonably be associated to actual occurrences in the world. Suppe seems to overlook what is involved in moving from mathematical abstract equations to the same equations affixed with an ingredient that allows associations with the world. Yet this element of identifying mathematical symbols with specific aspects of real systems can easily be accommodated into his picture. In fact, it is my contention that this understanding of the theory structure, namely the distinction between theory-induced and causally possible physical systems, captures an element of actual scientific theorising that van Fraassen’s picture obscures (*viz.* that particular aspect of ‘identification’ mentioned above). It is partly for this reason that I have earlier claimed that Suppe’s picture is more defensible than van Fraassen’s is. This distinction also has an additional consequence that renders it closer to actual science - the fact that it allows for the elements of abstraction and idealisation to emerge as significant features of scientific theorising. Before I begin to analyse these aspects of Suppe’s theory, I will enter into a brief digression to explicate the use of language.

⁴⁴ Giere introduces a useful distinction between *interpretation* and *identification*. ‘...[Interpretation] is the linking of the mathematical symbols with *general terms*, or concepts, such as ‘position’. ...[Identification] is the linking of a mathematical symbol with some feature of a *specific object*, such as ‘the position of the moon’.’ [Giere 1988, p75] I take it that *interpretation*, in Giere’s sense, is fixed by the semantic models, I also take it that in van Fraassen’s version, *identification* is presumably established by the satisfaction function that maps elementary propositions onto regions of the state-space. By no means do I mean to imply that in Suppe’s version of the SV the process of identification is exhausted at the stage of identifying a theory-induced with a causally possible physical system, or that it applies only to this stage of theoretical construction.

3.2.2.2 The Theory Formulation Language

According to Suppe we must distinguish between the theory and the linguistic formulation of the theory. A formulation of a theory, says Suppe, is a collection of propositions, which may consist of a few specified propositions together with all their deductive consequences that are true of the theory. These propositions typically constitute the *theory formulation language*, which is used not only to describe the theory but also to refer to physical systems and to phenomena within the theory's intended scope. To explicate this, Suppe makes use of the distinction in *strict* and *amplified usage* of propositions. Under strict usage, a proposition is used to describe, and refer to a particular system. For instance under such usage a proposition will be used solely in reference to the theory and not to phenomena. Under amplified usage, a proposition is used indifferently to describe, and refer to different systems. Amplified usage is such that the same proposition can simultaneously describe features of the theory as well as of physical systems and phenomena. Propositions in the theory formulation language must admit of amplified usage, so that theoretical predictions via physical systems can be related to experimental measurements. But since propositions that admit of amplified usage also admit of strict usage (not vice-versa), propositions may also be used strictly with reference either to the theory or to physical systems or to phenomena.

The key features of propositions in the theory formulation language can be discerned by investigating their strict usage in theories, or physical systems, or phenomena. Suppe's analysis of the relation between theory formulation languages and theories is in many respects similar to van Fraassen's analysis. We already noted that the values of a physical parameter p are given by a physical quantity q . A set of elementary propositions in the theory formulation language is utilised to express that p has value q at time t . An elementary proposition φ is true of a state s in the domain of the theory (or of a subset $h(\varphi)$ of the domain), if s has value q for parameter p at time t . The mapping h of elementary propositions to states or subsets of states of the theory is known as the satisfaction function. In accordance with some logic (e.g. Boolean algebra mod-2, for CPM), elementary propositions may be compounded together.

Together ‘...the set of propositions, the theory, the satisfaction function h , and the logic of the theory determine a *language of physical description*, which is a sublanguage of the theory formulation language’ [Suppe 1989, p89]. This sublanguage is capable of describing physically possible states of the theory and consequently the physically possible states of physical systems. The language of physical description together with an augmented logic peculiar to each theory, in which the laws of the theory can be expressed (e.g. differential equations in CPM), forms the theory formulation language.

We may recall that a physical system in the class of theory-induced physical systems may be construed as the restriction of the theory to a single sequence of states. It may therefore seem that propositions of the theory formulation language may be used to describe and refer to theory-induced physical systems. In other words, propositions that are true (or meaningful) of a particular physical system would also be true (or meaningful) of the theory. Although this would not mean that all propositions true (or meaningful) of the theory would also be true (or meaningful) of all physical systems. They would only be true (or meaningful) of some physical system, (e.g. a proposition true or meaningful of a two-body system in CPM cannot necessarily be so for a three-body system). Yet, as Suppe points out, the theory formulation language is not sufficient to characterise physical systems. The reason for this is that one of the aims of a theory is to exactly circumscribe the class of causally possible physical systems. If the class of theory-induced physical systems happens to be identical to the class of causally possible physical systems then the theory formulation language will suffice, since whatever proposition is true of a theory-induced physical system (hence of the theory) is also true of a causally possible one and conversely. The language, however, is the means by which we check for the identity of these two classes. In other words, there must be a way to establish that, if propositions are true of a causally possible physical system then they are also true or false of the theory and conversely. Notice that the logic of the theory formulation language restricts the ways in which elementary propositions may be compounded together. As a result, some propositions that may be true of causally possible systems are excluded from the theory formulation language, hence the falsity of the theory cannot be established because such counter-instances to the theory cannot be stated in that language. Suppe

proposes that an *expanded theory formulation language* (to which the theory formulation language is a sub-language) be used to describe all causally possible physical systems. He gives only a very general description of the elements of this language: The logic of the language should not impose any restrictions on the admissible truth-functional combinations of propositions, that it should possess an adequate mechanism ‘...to describe any logically possible behaviour of any logically possible physical system...’ [*Ibid.*, p91], thus resulting in an expansion of the deductive logical apparatus of such a language.⁴⁵

Finally, Suppe considers the strict usage of propositions in the suggested expanded theory formulation language with reference to phenomena. According to an account of factual truth that he develops in a separate work, the world consists of ‘particulars’.⁴⁶ The phenomena in the theory’s intended scope will be systems of these particulars, which possess intrinsic properties and enter into intrinsic relations that need not be observable. Suppe calls such systems, *phenomenal systems*, and further clarifies that each causally possible phenomenon is a *causally possible phenomenal system*. To relate to our earlier example, a phenomenal system would be an actual torsion pendulum apparatus consisting of an elastic rod connected to a massive object, which are the system’s observable object particulars. It must be added that to every such phenomenal system there corresponds a causally possible physical system of a given theory. Since elementary propositions assert that a physical parameter p has a physical quantity q at time t , and since p ’s are kinds of attributes that particulars may possess, then elementary propositions can be used to describe phenomenal systems, i.e. to refer to particulars and predicate attributes in phenomenal systems. Physical systems, it was mentioned, are relational systems that have a

⁴⁵ Although this is not a matter of concern and investigation in the present work, it does not seem to me an exaggeration to conjecture that these demands on an expanded theory formulation language are an indirect attempt to accord to scientific languages the descriptive power of set theory. It is therefore a laborious task to allow the subject matter of a theory to dictate the language formulation.

⁴⁶ I shall not examine his theory of factual truth. The interested reader may inquire into Suppe 1973. For the sake of my discussion, let me just emphasise that objects are particulars but particulars are not necessarily just objects. By particulars Suppe refers to existing things or substances, mental or physical, each of which possesses many characteristics like qualities, properties or relations.

domain of states and attributes (the physical parameters) defined over these states. Similarly phenomenal systems are relational systems with a domain of particulars with intrinsic characteristics such as properties the particulars possess or relations they enter into.

Through these considerations Suppe essentially attempts to assign to language a purely descriptive role. Just as for van Fraassen, the representation capacity is stripped away from language and shifted to the theory structure and the physical systems. In the SV, language is given the purely expressive role of defining the classes of mathematical structures that constitute the theory, and comparing these to structures about the phenomena that are described in the same language.

3.2.2.3 The Structural Relation Between Physical and Phenomenal Systems

Suppe's claim is that to each phenomenal system P corresponds a physical system S . I would like to address the sort of correspondence between P and S that Suppe suggests. It was mentioned earlier that theories characterise classes of phenomena in their intended scope, and that they do not do this by describing these phenomena in their full complexity. Instead, Suppe's understanding is that certain parameters are abstracted and employed in this characterisation. In the case of CPM, these are the position and momentum vectors. These two parameters are abstracted from all other characteristics that phenomenal systems may possess because they are assumed to be the only ones influencing mechanical systems. The notion of abstraction is here used in an Aristotelian sense, i.e. in the sense of subtraction or removal from.⁴⁷ Suppe

⁴⁷ There is the tendency among philosophers to confuse this notion of 'abstraction' with the Duhemian use of it. Cartwright, 1989 chapter 5, makes the effort to clarify the distinction between the two uses of the term. Duhem's use of the term is such as to address the idea of *symbolic* representations of real physical objects and their relations. The mere use of symbolic representations of the real imposes on scientific theories the 'medium' of abstraction. But this is not the use of the term by Cartwright, and neither is it by Suppe. Abstraction used in the Aristotelian sense '...means 'taking away' or 'subtraction [of]' ...' [Cartwright 1989, p197], some of those factors that may *influence* the concrete or some of the properties the concrete may possess.

however impels us to see the process of abstraction going one step further. Once the factors, that are assumed to influence the class of phenomenal systems in the theory's intended scope, have been abstracted (isolated) the characterisation of physical systems still does not *fully* account for phenomenal systems. Physical systems are not concerned with the actual values of the parameters the particulars possess, e.g. actual velocities, but with the values of these parameters under certain conditions that obtain only within the physical system itself.⁴⁸ Thus in CPM, where the behaviour of dimensionless point-masses are studied in isolation from outside interactions, physical systems characterise this behaviour only by reference to the positions and momenta of the point-masses at given times. Physical systems are therefore, '...highly abstract and idealised replicas of phenomena, being characterisations of how the phenomena *would have* behaved *had* the idealised conditions been met.' [Suppe 1989, p65] How is the correspondence between *P* and *S* to be understood? The *simple pendulum* can serve as an example to 'fit into', and by which to visualise, the correspondence between *P* and *S*; in other words to make sense of the *replicating* relation.

A theory-induced physical system has to be consistent with Newton's 2nd law, which is understood in Suppe's theory along definitional lines. A category of systems studied in CPM, known as *conservative*, assume that the total force on the system is

⁴⁸ These conditions are, in the literature, repeatedly termed as idealisations. Amongst a large number of authors who adopt this terminology some are Giere 1988a, Shapere 1984, McMullin 1985, Laymon 1985, 1995, Morrison 1997, 1998 and forthcoming (a). These authors have been primarily concerned with questions of *how* idealisations are used in science. As a consequence, they do not explicitly make the distinction between *idealised manipulations of theories*, as opposed to *theories propounded as idealisations*. The only authors to my knowledge that explicitly pursue the latter analysis are Suppe and Cartwright 1983, 1989, 1989a, 1992. In attempting to analyse theories as idealisations one must address questions as to *what* idealisations are and *what kinds* of idealisations are employed in the construction of scientific theories, and finally confront the question of *how* theories propounded as idealisations may be related to phenomena. Suppe is a starting point but as I shall be arguing his analysis is incomplete and subsequently inadequate. It must be added that the aforementioned authors use the term 'idealisations' partly because they do not make the above distinction. Once the distinction is recognised a more general and accurate term to use is *abstractions*, as I will be claiming in the discussion in chapter 6.

conservative. This is expressed in the mathematical formalism by $\nabla \times \mathbf{F} = 0$, which is a necessary and sufficient condition for the force to be conservative. Within the physical understanding of the formalism this amounts to the total energy of the system being conserved. For conservative forces it can easily be shown that the force relates to the potential energy of the system in the following way: $\mathbf{F} = -\nabla V$. In the one-dimensional case this equation reduces to the well known relation $F(\xi) = -dV(\xi)/d\xi$, where ξ is the generalised coordinate.

The linear harmonic oscillator, assumed initially to be a conservative system, is under the influence of a *linear restoring force*. This is understood to imply that the force function has the form $F = -k\xi$, where k is a positive constant; or in accordance with the above relation, that the potential energy function is of the form $V(\xi) = \frac{1}{2}k\xi^2$.⁴⁹ Applying Newton's 2nd law to this force function yields the equation of motion mentioned in section 3.2.2.1, namely

$$\ddot{\xi} + (k/m)\xi = 0 \quad (3.1)$$

The preceding equation of motion applies to the one-dimensional motion of a particle, which is displaced from its stable equilibrium position. It is therefore a particular application of Newton's 2nd law that we may refer to as a theory-induced physical system following Suppe, or simply view it as a semantic model that satisfies the laws of the theory. To repeat an important claim of the SV, such models can be directly defined without recourse to syntax.

Any phenomenal system whose mathematically formulated dynamics yield an equation of motion identical in form to equation (3.1) qualifies as a linear harmonic oscillator. We have already seen that examples of such are the torsion pendulum and the mass-spring system. Nevertheless, there are plenty of other phenomenal systems

⁴⁹ Harmonic variations occur outside of CPM, e.g. the classical electromagnetic theory and quantum mechanics. In quantum mechanics, for instance, the use of the concept of a linear restoring force would be of no help, instead in setting up the Schrödinger equation for such systems the notion of energy proves to be more helpful. In fact so is the case in CPM, where, as is well known, using the Lagrangian or Hamiltonian formalisms to set up the equations of motion proves to be a way to avoid the mathematical complexities that the use of the appealing notion of force gives rise to.

that although they do resemble the linear harmonic oscillator they do not fit it, unless additional assumptions are made. In the simple pendulum case, we have an example of two-dimensional periodic oscillations. The result is the following equation of motion, given in terms of the angle of oscillation variable and its 2nd time-derivative:

$$\ddot{\theta} + (g/l)\sin\theta = 0 \quad (3.2)$$

An exact analytic solution to equation (3.2) involves the Jacobian elliptic sine function. This leads to an expression for the period of oscillation in terms of an elliptic integral, which can be quite unmanageable and hence a perturbation expansion of the integrand is needed for a convenient general solution. But we can simplify the problem by reducing it to the one-dimensional case, thus obtaining a simple analytic solution. By assuming that the amplitude of oscillation θ is infinitesimally small (which is another way of saying that the displacement of the bob is one-dimensional) then $\theta \approx \sin\theta$, and equation (3.2) reduces to equation (3.1) in terms of θ . The process of implementing this assumption is part of the process of developing the particular theory-induced physical system, which finds its causally possible analogue in the simple pendulum modulo this assumption together with the assumptions for a conservative force, a uniform gravitational field, and an extensionless point-mass. Some physical implications of the four assumptions involved are well known. The pendulum will continue to oscillate indefinitely since it is assumed that there is no energy dissipation, and it will do so under the influence of a constant downward force since it is in a uniform gravitational field. Both of these together imply that the period of oscillation is constant.

However, in the actual phenomenal system we know that such a situation does not occur. The actual pendulum apparatus is subject to a number of different factors (or may have a number of different characteristics) that may be divided into those influencing and those not influencing the process of oscillation, i.e. the mechanical process under study. Those factors that we assume to influence its oscillations can be further categorised into those *internal* to the system and to those *external* to it.⁵⁰ Some internally influencing factors *inter alia* are the amplitude of the angle of

⁵⁰ These categorisations are my suggestion and not Suppe's.

oscillation, the mass distribution of the bob and suspension wire, the wire connections and the flexibility of the wire. Some externally influencing factors *inter alia* are the gravitational field of the earth (which is assumed to be uniform), the buoyancy of the bob, the resistance of the air and the stirring up of the air due to the oscillations. Some aspects of the apparatus that are assumed not to influence the process of oscillation because they do not depend upon the mass, the velocity or the displacement, are for instance the colour of the bob or the illumination of the experimental set-up. In modelling the simple pendulum by means of the linear harmonic oscillator what is involved, in addition to the obvious approximation ($\theta=0$), is abstracting the pendulum from factors assumed to influence the oscillations in a similar manner as from those assumed not to. Therefore, the replicating relation between P and S that Suppe urges cannot be understood as one of identity or isomorphism or the like. Suppe is explicit about this:

‘The attributes in S determine a sequence of states over time and thus indicate a possible behavior of P (i.e., a sequence of changing attributes the particulars in P could have at various times). Accordingly, S is a kind of *replica* of P ; however, it need not replicate P in any straight-forward manner. For the state of S at t does not indicate what attributes the particulars in P possess at t ; rather, it indicates what attributes they *would have* at t *were* the abstracted parameters the only ones influencing the behavior of P and were certain idealised conditions met. In order to see how S replicates P we need to investigate these abstractive and idealising conditions holding between them.’ [1989, p94]

To summarise, the replicating relation is counterfactual: if the conditions assumed to hold for the construction of the theory-induced physical system were to hold for the phenomenal system, then and only then would the phenomenal system behave in the way described by the theory-induced physical system. The key notion is that of abstraction. It was hinted upon earlier that in selecting a few parameters by which to characterise the phenomena, an ‘isolation’ of the theoretical domain (or the scope of the theory) is achieved. By means of this kind of abstraction the factors that are removed are those that are assumed not to influence the (in our case, mechanical) phenomenal systems. But there is a second kind of abstraction that Suppe tacitly employs in his theory, where a number of features that are assumed to influence the values of the parameters used are also removed. In the simple pendulum example some of these were mentioned above and divided into those internal and external to the system. The removal of these features from S is done via the foregoing four assumptions. Obvious examples are, that the assumption with regard to the non-

dimensionality of a body is the result of abstracting from its actual dimensions, and the assumption with regard to a conservative force is the result of abstracting it from its surroundings (i.e. conceptually isolating the system from outside interactions). Hence, in removing the actual dimensions of the pendulum and consequently the actual mass distribution, and in ignoring the retarding effects the medium has on the oscillations, the result will be a discrepancy between the theoretical prediction and the experimental measurement. That is, the values of the parameters characteristic of the state of the physical system S at time t will not be the actual values characteristic of the phenomenal system P . I believe however, and subsequently I will argue, that Suppe stops short without recognising that some of the initially abstracted features are subsequently re-introduced into our theoretical descriptions. Although Suppe points in the right direction and discerns the features of abstraction and idealisation involved in our theorising, he fails to recognise that if our epistemic limitations permit it, we extend a model of the theory by adding the influencing abstracted features back into our theoretical description. This, of course, has an effect on the representation relation he advocates, because we need to establish how the resulting theoretical construction that contains the addenda (i.e. the representation model) links to the theory. Moreover, it has an effect on the understanding of scientific theories advocated by the SV, because we need to establish how these constructs relate, if they do, to the theory structure.

Despite these disputes with his view, I recommend that we learn from Suppe. He invites us to distinguish between two ways the counterfactual replicating relation could be true: The case of pure abstraction, where ‘...it may be causally possible for P to realise the conditions [assumed to occur in S] such that P ’s behaviour would be as S indicates’ [*ibid.*, p95]. For instance under satisfactory experimental conditions, the actual physical pendulum may be isolated from the effects of air resistance or possibly other outside influences. The other case, a special kind of abstraction, is that of pure idealisation where ‘...it may be such that it is causally impossible for P to realise the conditions [assumed to occur in S] such that P ’s behaviour would be as S indicates’ [*ibid.*, p95]. An example is the assumed nonexistence of the body’s extension or dimension. This distinction and the consequent definitions evidently lead to the understanding that, ‘abstractions’ can be replicated to a satisfactory degree in

the laboratory, whereas ‘idealisations’ cannot.⁵¹ I choose to understand Suppe’s definitions, offered to distinguish between abstraction and idealisation, as follows. Idealisations are the conceptual means of distorting the phenomena as to reach the goal of mathematical simplicity, whereas abstractions are the conceptual means by which to confine theoretical descriptions to a limited number of properties or influencing factors. The latter are not distortions since the ignored properties can potentially be introduced into the description, despite the fact that Suppe ignores this.

3.2.2.4 Theoretical Representation by Means of Relational Systems

In figure 3.2 I present a schematic of Suppe’s SV account. As for van Fraassen’s version, the chosen theory language defines a mathematical structure. By means of the theory attributes we can single out certain sequences of states from this structure, called theory-induced physical systems. These sequences of states are those that are physically possible (as explained in section 3.2.2.1) and they are made available for modelling the domain of the theory. What distinguishes Suppe’s understanding of the state-space is that the physical systems are (in his view) abstract and idealised replicas of real systems, and that one of the main objects of the theory is to circumscribe the class of causally possible physical systems.

Experimentation on the real or phenomenal systems involves once again the use of procedures by which the raw data are transliterated into structures expressed in the theory language. In addition however, Suppe claims that in constructing data models the abstractions and idealisations involved are partially compensated. This is done either by doing the experiment in highly controlled and isolated circumstances thus ideally eliminating the unwanted influencing factors, or by determining the effects or influences of factors not accounted for in the physical systems. These influences are then accommodated into the data models by converting the raw data into what they

⁵¹ It is, I hope, evident that I regard Suppe’s theory of abstraction and idealisation as a good starting point, although I find it inadequate for the purposes of understanding actual scientific practices. But this discussion has to be postponed for now, I shall return to it in Chapter 6.

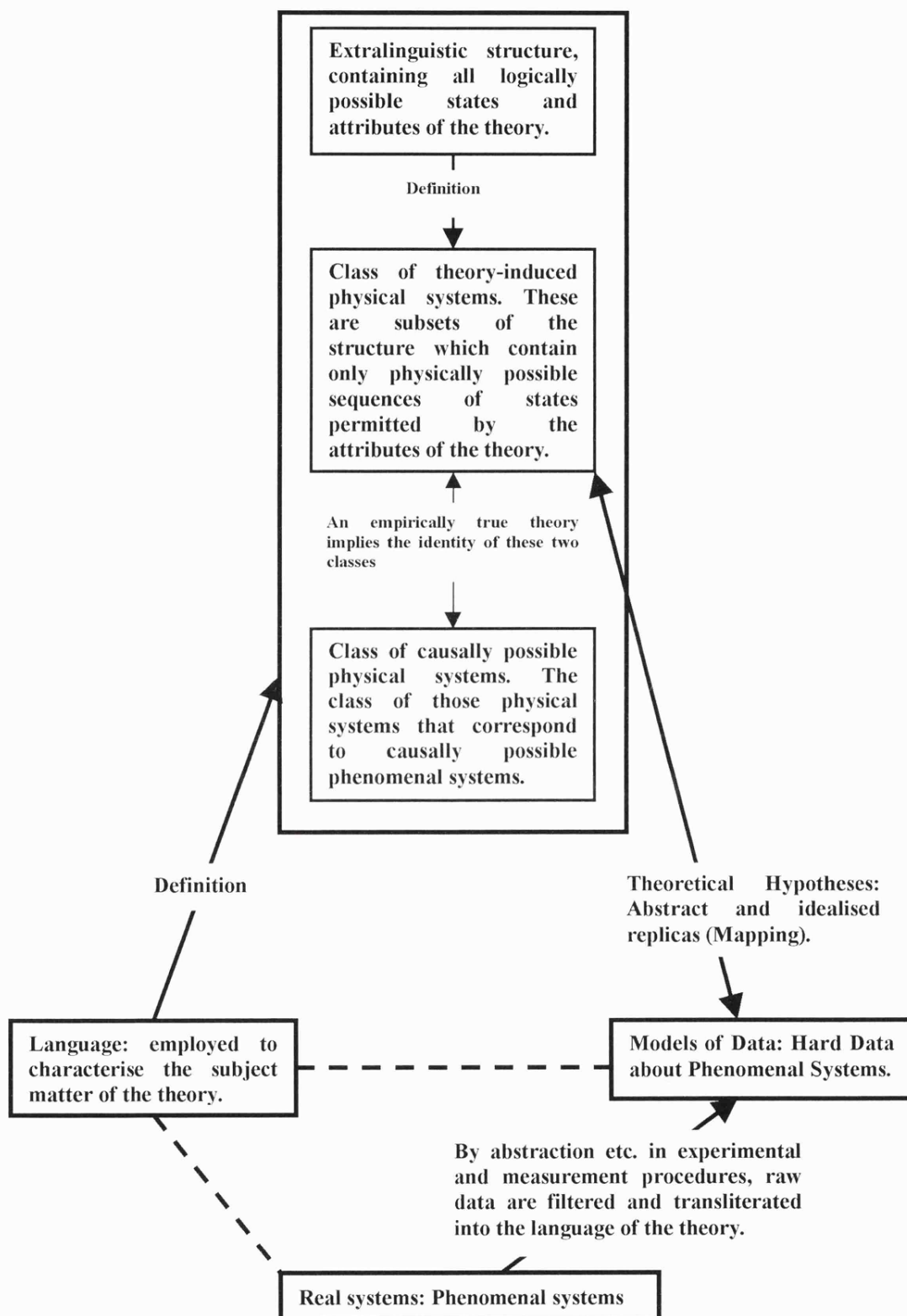


Figure 3.2

would have been had these influences not existed. The resulting data models contain empirical data about the corresponding physical systems.

The divergence of Suppe's view from that of van Fraassen is one based primarily on the representation relation of theory to phenomena. Suppe understands the theory (i.e. the state-space) as being a highly abstract and idealised representation of the complexities of the real world. Van Fraassen disregards this because he is concerned with the observable aspects of theories and assumes that these can, to a high degree of accuracy, be captured by experiments. Thus van Fraassen regards theories as containing empirical substructures that stand in an isomorphic relation to the world or more accurately, the observable aspects of it. Suppe's understanding of the theory-structure, however, points to a significant drawback present in van Fraassen's view: How can we justify the claim that the data model is isomorphic to an empirical substructure of the model or theory, given that the data model is a selective and refined form of an experimental report derived from an experiment that takes place in highly idealised circumstances? Furthermore, even in the case when a data model is in fact isomorphic to an empirical substructure, it is so because the data model is converted to what the measurements *would have been* if the influences that are not accounted by the theory did not have any effect on the experimental set-up. Suppe's quarrel with van Fraassen's view of the representation relation is not just about how distant from actual scientific practices this view is. But also about the fact that if and when an isomorphic relation obtains the only epistemic inference we can draw is that the data model of a highly idealised experiment is isomorphic to an empirical substructure. This is a significantly different claim from what van Fraassen would urge, i.e. that the world or some part of it is isomorphic to an empirical substructure of the theory.

According to Suppe's understanding of the theory-structure no part of the world is or can be isomorphic to a model of the theory, because of the elements of abstraction and idealisation that are involved in our theorising. It is primarily for this reason that I consider Suppe's view as the most defensible version of the SV. Nevertheless, the same assumptions that underlie van Fraassen's version, which were outlined in Section 3.2.1.3, underlie Suppe's version too. Suppe realises that abstraction and

idealisation are significant features of scientific theorising, yet he does not go far enough as to explore them. In order to see the nature of the replicating relation between the model of the theory and the real system, he invites us to investigate the abstractive and idealising conditions that hold between the two. However, the kind of investigation he presumably refers to is a logical one, one that would establish the sort of realism he advocates. Chained to his structuralist analysis of theories, Suppe is fails to see that -and how- actual representation models in science involve the process of linking the highly abstract and idealised concepts of the theory to the real system. This is a process that functions conversely to the process of abstraction, which allows us to bring our theoretical descriptions closer to the concrete real systems we encounter in the world. In Chapter 6, I shall label this process the process of concretisation. In order to analyse the concretisation process we must look into the construction of representation models. Of course, Suppe fails to discern the importance of this process because for the SV the linking between theory and experiment consists simply in a mapping of structure. An actual representation model employed in scientific inquiry has no special status for the SV, it is in principle reducible to some model of the theory.

We could put what Suppe teaches us in a rather crude manner: ‘here is the theory, what is done is done, now it is the job of the experiment to meet the demands of the theory where it can’. Thus, despite the fact that Suppe discerns highly significant features of scientific theorising (i.e. abstraction and idealisation), the framework of the SV inside which he operates leads him to obscure a most important aspect of theory application, that of representation model construction via concretisation. Just as for van Fraassen’s version, so too for Suppe’s version of the SV, the application of a theory just consists in determining which model of the theory (i.e. theory-induced physical system) is best suited for representing a particular phenomenal system. This view, as we shall see in the Chapters that follow, is remarkably distant from actual scientific practices, despite the wishes of its proponents.

3.3 Clarifications on the Notion of ‘Model’ Inherent in the Semantic View

Now according to the SV, theories are families or classes of models. A commonplace and prevalent question is what sense is attributed to the notion of ‘model’. It is true that in the early stages of the development of the SV its proponents inadvertently use the term ‘model’ in the logician’s sense of the notion, that is, as an interpretation that satisfies a set of statements. We find for instance Suppes advocating this sense:

‘Our references to models in pure mathematics will, in fact, be taken to refer to mathematical logic, that branch of pure mathematics explicitly concerned with the theory of models. The technical notion of possible realization used in Tarski’s definition need not be expounded here. Roughly speaking, a possible realization of a theory is a set-theoretical entity of the appropriate logical type.’ [Suppes 1961, p166]

He continues in the next paragraph:

‘A possible realization of the axioms of classical particle mechanics, is then an ordered quintuple $\wp = \langle P, T, s, m, f \rangle$. A model of classical particle mechanics is such an ordered quintuple. It is simple enough to see how an actual physical model in the physicist’s sense of classical particle mechanics is related to this set-theoretical sense of models.’ [*Ibid.*, p167]

Van Fraassen also advocates the same sense:

‘There are natural interrelations between the two approaches [i.e. the RV and the SV]: an axiomatic theory may be characterized by the class of interpretations which satisfy it, and an interpretation may be characterized by the set of sentences which it satisfies; though in neither case is the characterization unique. These interrelations, and the interesting borderline techniques provided by Carnap’s method of state-descriptions and Hintikka’s method of model sets, would make implausible any claim of philosophical superiority for either approach. But the questions asked and methods used are different, and with respect to fruitfulness and insight they may not be on a par with specific contexts or for special purposes.’ [van Fraassen 1970, p326]

And finally Giere and Suppe are also tied-up with such an apprehension of ‘model’:

‘I suggest calling the idealized systems discussed in mechanics texts ‘theoretical models’ or, if the context is clear, simply ‘models’. This suggestion fits well with the way scientists themselves use this ...term. Moreover, this terminology even overlaps nicely with the usage of logicians for whom a model of a set of axioms is an object, or a set of objects, that satisfies the axioms. As a theoretical model, the simple harmonic oscillator, for example, perfectly satisfies its equation of motion.’ [Giere 1988, p79]

‘This suggests that theories be construed as propounded abstract *structures* serving as models for sets of interpreted sentences that constitute the linguistic formulations. These structures are *metamathematical models* of their linguistic formulations, where the same

structure may be the model for a number of different, and possibly nonequivalent, sets of sentences or linguistic formulations of the theory.' [Suppe 1989, p82]

From this usage of the notion of model, one is justifiably led to believe that propounding and identifying a theory as a class or family of models, without recourse to its syntax, only aims at convenience in avoiding the hustle of constructing a standard formalisation, and at adaptability of our reconstruction with common scientific practices. In short, the difference -between the SV and the RV- is methodological and heuristic. At the same time, the standard problems associated with the syntax (observation/theory distinction etc.) disappear, one would say almost miraculously. Such remarks by the proponents of the SV, as the above, have led some authors to question the logical difference between defining the class of models directly as opposed to metamathematically.

The latter approach was taken by Friedman and by Worrall in their separate reviews of Van Fraassen (1980). Their argument is similar. They ask whether the class of models that constitutes the theory according to the Semanticists, is to be identified with an 'elementary class', i.e. a class that contains precisely the models of a theory formalised in first-order language. They both notice that not only does Van Fraassen offer no reason to oppose such a supposition, but he even encourages it (as in the above quotation). But if that is the case,

'[t]hen the Completeness Theorem immediately yields the equivalence of van Fraassen's account and the traditional syntactic account [i.e. that of the RV].' [Friedman 1982, p276]

In other words,

'So far as logic is concerned, syntax and semantics go hand-in-hand -to every consistent set of first-order sentences there corresponds a non-empty set of models, and to every normal ('elementary') set of models there corresponds a consistent set of first-order sentences.' [Worrall 1984, p71]

If we assume (following Friedman and Worrall) that the Semanticists are referring to the 'elementary class' of models then the preceding argument is perfectly sound. The SV, in agreement with the Logical Positivists, retains formal methods as the primary tool for philosophical analysis of science. The only new element of its own would be the suggestion that, rather than developing these methods using proof-theory we should instead use formal semantics (model-theory). But van Fraassen is of a

different opinion. On two different occasions he resists the construal of the class of models of the SV with the ‘elementary class’.⁵² What follows is a rehearsal of his argument.

The SV claims that to present a theory is to describe (define) a set M of models. This is the class of structures the theory makes available for modelling its domain. The most likely mathematical object to be included in this class is the real number continuum. Now his argument goes, if we are able to formalise what is meant to be conveyed by M in some appropriate language, then we will be left with a class N of models of the language, i.e. the class of models in which the axioms and theorems of the language are satisfied. Our hope is that every structure in M occurs in N . However, on the one hand the real number continuum is infinite and ‘[t]here is no elementary class of models of a denumerable first-order language each of which includes the real numbers. As soon as we go from mathematics to metamathematics, we reach a level of formalisation where many mathematical distinctions cannot be captured... The moment we do so, we are using a method of description not accessible to the syntactic mode.’ [van Fraassen 1987, p120] On the other hand, ‘[t]he Löwenheim-Skolem theorems ...tell us... that N contains many structures not isomorphic to any member of M .’ [van Fraassen 1985, p302] It is so because he relies on the following reasoning: The Löwenheim-Skolem theorem tells us that all satisfiable first-order theories that admit infinite models will have models of all different infinite cardinalities. Now models of different cardinality are non-isomorphic. Consequently every such theory will have models that are not isomorphic to the intended models (i.e. non-standard interpretations) but which satisfy the axioms of the theory. Thus van Fraassen is telling us that M is the intended class of models, and since the limitative meta-theorems tell us that it cannot be uniquely determined by any set of first-order sentences we can only define it directly. His final remark makes the suggested understanding clearer:

‘The set N contains ...[an] image M^* of M , namely, the set of those members of N which consist of structures in M accompanied by interpretations therein of the syntax. But, moreover, ... M^* is not an elementary class.’ [van Fraassen 1985, p302]

⁵² He attributes this misunderstanding to being at the time overly impressed with the completeness theorem for quantificational logic. See van Fraassen 1985, pp301-303, and his 1987.

Evidently, van Fraassen's argument effectively aims to establish that the purportedly defined class of models is not an 'elementary class'. Nevertheless, his argument has not been fully convincing to some sceptics, such as Schaffner (1993) who claims that the difference between the RV and the SV approaches to scientific theories is primarily heuristic and methodological. I think that Schaffner's reservations towards the SV may be right but not for the reason he claims. It seems that defining a class of mathematical structures (which is not the 'elementary' class), the intended models, can only be done directly without recourse to any syntax. Hence the logical difference between the SV and the RV is distinct (despite Friedman's, Worrall's and Schaffner's reservations). In addition, the SV is heuristically and methodologically more useful, because of its emphasis on models as the vehicles of theoretical representation in science. Nevertheless, this is not a sufficient reason for an adequate account of theories. The important question for me is, despite the fact that the SV is heuristically and methodologically a useful approach, what implications does it have which make it an inadequate account of scientific theories? To address this question we must examine two of the SV's implications and their coalescence. The first is the obvious implication (which has been explicit throughout this Chapter), that all the models must be united under a common structure. The second is that the notion of model employed in the SV has two hermaphrodite functions, it is both an interpretation and a representation device. Before addressing my objections to these implications of the SV, let me first take a closer look at the second.

If we try to look at the notion of 'model' *simpliciter* as employed by the SV, the immediate question to ask is: Must we continue to think of these individual models, in M , qualifying as semantic models (i.e. that they are interpretations in the Tarskian sense of the abstract mathematical formalism)? I think the answer is yes. If we read the preceding remarks of the Semanticists without *identifying* the class M with N , then it is clear that this understanding follows naturally. But there is, I believe, another reason stemming from how the Semanticists construe 'family of models'. To say that we propound a theory by defining directly a family of models raises the question of how we are to construe the character of the family relation? Do we construe it as a class by virtue of the fact that all its individual members satisfy a set

of equations, or by virtue of the fact that its individual members demonstrate some kind of resemblance -without necessarily sharing a common characteristic or in some cases having conflicting characteristics between them?⁵³ But the latter construal cannot fit into the SV picture of theories; since, for instance, it would imply that a model that consists of both classical and quantum mechanical features can belong to the same family structure as a purely quantum mechanical model. But since the quantum mechanical features cannot be represented in a classical phase space, the mere idea of a uniting state-space conflicts with such a construal. The latter construal could also imply that we classify families of models by virtue of the fact that their members have for example resembling dynamics. Thus, models representing a linear restoring force, whether a classical or a quantum mechanical linear harmonic oscillator, belong to the same family. Once again the idea of a common state-space structure conflicts with such a construal. Therefore, the family membership proposed by the SV could only be the former, namely one based on the satisfaction of the laws of the theory.

Furthermore, with such a construal for family membership we could make sense of the role of 'embedding' of structures. Van Fraassen says that '...one structure is embedded in another, if the first is isomorphic to a part (substructure) of the second. Isomorphism is of course total identity of structure and is a limiting case of embeddability: if two structures are isomorphic then each can be embedded in the other.' [van Fraassen 1980, p43] Thus embedding also plays a part in determining family membership. A model belongs to the family of models that is identified with the theory, if it is embedded in a wider and more general structure, that of another model or more generally of the theory. It is hard to make sense of the notion of embeddability of one model in another or in the theory, other than by the satisfaction of the laws of the theory for specified parameters by the former.

Subsequently, what would be the next natural question to ask, namely 'how is the class of structures, that is made available for modelling the domain of the theory, to

⁵³ Hendry 1997, makes a similar point, he calls something similar to the former an 'intentional construal' and something similar to the latter an 'extensional construal'.

be defined?', finds its answer in the foregoing construal of family membership. Although not in an explicit manner, this question has been addressed and by now it may seem trivial: The function of the laws of the theory is to define the theory-structure and hence the models of the theory.

The best way to proceed to demonstrate this procedure (of defining the models) with clarity is by reference to an example. I think further indications to this construal, that models are the set of mathematical objects defined by means of the laws of the theory, are to be found in the work of another proponent of the SV, who begins with the following thesis and proceeds to apply the state-space approach to Evolutionary Theory:

'Under the logical positivist approach, formulation of the logical calculus involves viewing the theories as sets of statements. Interpretations that make all the statements in the set true -logicians call these 'models'- may be given for certain theories. In our discussion, a *model* is not such an interpretation, matching statements to a set of objects that bear certain relations among themselves, but the set of objects itself. That is, models should be understood as *structures*; in almost all the cases I shall be discussing, they are mathematical structures, i.e. a set of mathematical objects standing in certain mathematically representable relations.' [Lloyd 1988, p15]

Let me instead consider as an example Hamilton's equations of motion for a CPM system, whose solutions describe the development of a system in time, i.e. they function as the laws that govern the system:

$$\frac{\partial x}{\partial t} = \frac{\partial H}{\partial p} \quad \text{and} \quad \frac{\partial p}{\partial t} = -\frac{\partial H}{\partial x} \quad (3.3)$$

Evidently, we require a specification of the Hamilton function $H=T+V$ in order to solve the above equations, where T stands for the kinetic energy and V for the potential energy of the system. Each time we specify a Hamilton function, for a particular system, we define a model. Such a model is a mathematical structure that belongs to the phase space of CPM. Suppose our system is the linear harmonic oscillator, which can be represented by the Hamilton function:

$$H = \frac{p^2}{2m} + \frac{1}{2}kx^2 \quad (3.4)$$

This function is the analogue of a linear restoring force in the Newtonian formalism of CPM. So far, we still continue to regard x and p as just mathematical functions of t , and m and k as just constant parameters. The mathematical solutions of Hamilton's

equations yield elliptical spirals along the t -dimension in the 3-dimensional space of x - p - t . Projecting this space onto the two-dimensional space of x - p yields an ellipse.⁵⁴ The parameters m and k together with specified values of x and p for given values of t determine a class or family of mathematical structures -to put it geometrically, a family of ellipses. There is an indefinite number of possible ellipses, representing all possible trajectories (sequences of states) in the x - p state-space.

When the variables and parameters (i.e. the theoretical terms like mass and momentum) are interpreted (as a first step) as follows, x is the position and p the momentum of a one-dimensional oscillating point-mass of value m , we have the ‘mathematical image’ of a system performing linear harmonic oscillations that behaves according to Hamilton’s equations of motion.⁵⁵ As a second step, we identify the mass m with a particular body and the constant k with a particular characteristic of an oscillating physical system.⁵⁶ Finally given proper -normally, experimental-procedures we assign values to m and k , specific to the actual system of concern, and we can empirically test whether our model is an ‘acceptable’ representation of the physical system. Accordingly, a ‘model’ is a structure given by the equations of motion when supplied with a Hamilton function and an interpretation, as described. Thus, it seems to me that the sense of ‘model’ advocated by the proponents of the SV

⁵⁴ In the Hamiltonian formalism of CPM the phase space plays the role of a geometrical description of mechanical systems. Physicists often refer to this as a geometrical interpretation, however I think that it is just the result of the customary ways of geometrically describing mathematical equations. In general it is a space of $2n$ dimensions that correspond to the n generalised coordinates and n generalised momenta. The phase space representation of the system can be achieved given Hamilton’s equations for the particular system-Hamilton function. Each point in the phase space corresponds to a *definite* state of the mechanical system in question, assuming that the system is represented by the chosen Hamilton function. Hence *definite* knowledge of the Hamilton function is required for the points of the phase space to correspond to *definite* states of the system.

⁵⁵ In the actual definition of a model the distinction between the mathematical definition of the structure and this interpretation procedure are indistinguishable, i.e. the interpretation is part and parcel of the definition of the structure. Distinguishing the two serves only to recognise that models defined as abstract mathematical structures, by means of the laws of the theory, are indeed semantic models.

⁵⁶ The notions of ‘interpretation’ and ‘identification’ are clearly used here in Giere’s sense, see Giere 1985 and 1988.

has a dual character. On the one hand, it is used in the sense of an interpretation of a set of sentences, thus providing an interpretation of the abstract mathematical formalism. On the other hand, it is used as a representation device, that is, a mathematical structure, which after its mathematical terms are identified with particular characteristics of a physical system, is proposed for the potential representation of that system. I also believe that this understanding that I attribute to the proponents of the SV accords well with Giere's distinction between interpretation and identification of mathematical terms. I shall follow Giere's terminology and refer to such models, which have the attributes intended by the proponents of the SV, as 'theoretical models'.⁵⁷ In principle there is an indefinite number of mathematical definitions of a Hamilton function, hence not only is there an infinite number of logically possible theoretical models but there is also (in principle) an indefinite number of families of models (i.e. structure-types). These considerations reveal that the SV relies on the assumptions I indicated in sub-section 3.2.1.3, and which I recapitulate in the following final Section of this Chapter.

3.4 General Remarks on the Semantic View

Both the Received and the Semantic views employ formal methods for the philosophical analysis of scientific theories. There are however, significant differences among the two views. One such difference is of heuristic nature. In the SV the vehicles of representation are the models of the theory, and the laws of the theory function only as 'defining devices' of the models (hence they only hold in the sphere of the mathematical structure). The models of the theory are directly defined by the laws of the theory, and are thus united under a common mathematical structure, the state-space. In this work it will be assumed and not disputed -for the obvious reason that equations satisfy a structure- that theories can be presented in terms of mathematical structures. If the SV were offered as a *rational reconstruction* of scientific theories, i.e. an explication of the structure of theories, it would mean

⁵⁷ This notion is, in my view, very close to Suppe's notion of 'theory-induced physical system', if the fact that physical systems refer to a specific sequence of states is ignored.

that it did not aim to describe how actual theories are formulated, but only to indicate a logical framework into which theories can be essentially reformulated. This would only imply that the SV presents a canonical structural formulation for theories and claims that any theory can be given an essentially equivalent reformulation in this canonical way. Nonetheless, to *identify* the theory with a structure (i.e. to construe the equations extra-linguistically) is in addition, to assume that the SV account of representing phenomena (i.e. the reduction of theory representation to a structural relation and the subsumption of all representation models under a unifying theory structure) is legitimate. It is this philosophical position that I wish to oppose in this work. I have tried to emphasise the fact that not all Semanticists share the same views on the representation relation. In particular van Fraassen wants an isomorphic relation between -at least the observable aspects of the- theory and experiment, whereas Suppe understands theories as being abstract and idealised replicas of phenomena. It may therefore seem that particular criticisms will not necessarily inflict ‘discomfort’ on both views. I will try to show, however, that in tackling the underlying assumptions of the reduction of theory representation to a structural relation will indeed inflict discomfort on both views. My criticism of the SV, which will follow in the next Chapters, is primarily motivated by my inability to accept the justification of, and my lack of satisfaction with, these assumptions. Although I find the SV a powerful heuristic tool due to its emphasis on models in scientific inquiry, I do not see this as adequate reason to accept its explication of the nature of scientific theories.

I want to repeat the assumptions that, in my opinion, underlie the SV of scientific theories, in order to set the foundation of my arguments in the coming Chapters. The SV assumes that there can be a sharp distinction between models of theory and models of data, and that when we define a theory-structure we immediately lay down an indefinite number of models that are antecedently available for modelling the theory’s domain. It also assumes that the methods and processes of construction of actual representation models can be ignored, for they bear no effect on how these models function in scientific inquiry. The latter assumption is accompanied by the mistaken view that modelling in science, i.e. the construction of representation models, is only done by having a model of the theory as a starting point. Finally, it assumes that the actual scientific models used in theoretical representation

approximate, in one form or another, a model of the theory, or that they are the ‘pragmatic counterparts’ of corresponding models of the theory.⁵⁸

If our analysis of theories and theory-application is to give the right emphasis on models, these assumptions must be avoided. In doing so, however, we need to give representation models the special status (in scientific inquiry) of ‘mediators’ between theory and experiment. Also we need to restore the capacity of ‘representation’ to language, and in particular to the element of *denotation* that linguistic entities, such as mathematical equations, possess. In this work I do not intend to explore the latter position in any explicit and elaborate manner, as I have no intention of working out a ‘theory’ of theoretical representation. My primary focus in the remaining Chapters of this work is the former idea: what roles do representation models play, in scientific inquiry, that the SV obscures?

In Section 3.3 I urged the claim that the inadequacy of the SV to account for scientific theories lies primarily in two of its implications. Firstly, it employs a notion of ‘model’ that has two functions -interpretation and representation. Secondly, it wants models functioning in these two ways to be united under a common structure. In the remaining of this work I will be arguing that to hold both of these positions simultaneously is untenable. We have seen that the devices by which the theoretical models are defined are the laws of the theory. Hence the laws of the theory provide the constraints which determine the structure of these models. Now it is not hard to see that models viewed as interpretations (in a Tarskian sense) are united under a common structure that may be determined by the laws of the theory. What is problematic, however, is the fact that the SV assumes, firstly that models functioning

⁵⁸ ‘Pragmatic counterpart’ is used in the above context to refer to van Fraassen’s view that the notion of ‘approximation’ does not require further analysis within a structuralist analysis of scientific theories. I therefore take it that the notion of ‘approximation’ could be eliminated from some versions of the SV. This point is clarified at the beginning of Chapter 4. In addition, the use of the term ‘pragmatic counterpart’ is associated with van Fraassen’s use of ‘pragmatic’, which enters into his discussion of how science only represents the structure of nature, that appears to commit him to a pragmatic view of model choice and theory choice. The latter view, which is briefly explained in sub-section 4.1.1, is evidently not shared by all proponents of the SV.

as interpretations are also representation models, and secondly that models functioning as representations can be united under a common structure in such a trivial way. When we construct representation models we continuously impose constraints that alter their initial structure. The departure of the resulting constructs from the initial structure is such that it is no longer possible to consider them all as united under a common structure.

It is helpful to discern two intertwined general strategies by which to investigate this weakness of the SV and its inadequacy to capture the roles played by representation models in scientific inquiry. The first is to attempt to show that actual scientific models, and the ways they are employed for particular representational purposes, reveal that they cannot be viewed as families of theoretical models in the way the SV purports. This is a twofold strategy, the first aspect of which is that the representation models used in science do not always qualify as theoretical models (i.e. interpretations); and its second aspect is that the representation models cannot legitimately be grouped under one structure (theory). The second strategy involves showing that a simple mapping of the models of data onto theoretical models (or parts of them) is not an adequate way to explain the theoretical representation of phenomena. Indeed it simplifies and subsequently obscures both actual scientific practices and the process of interpretation involved in actual practices. It is in this direction that I will now turn. In the Chapters that follow I shall employ both of these strategies without attempting to make them explicit and to keep them distinct.

4 Representation Models in Classical Mechanics

4.1 Untenable Implications of the Semantic View

It was shown in Chapter 3, that Hamilton's equations of motion for each Hamilton function could be represented structurally by means of phase space configurations (i.e. the two dimensional x - p space comprising of elliptical trajectories). If it were the case that we would be able to solve Hamilton's equations for whichever Hamilton function, we would have a 'complete' description of CPM systems (and possibly of all our theories) in terms of the phase space, *available for modelling* (hence representing) all possible physical systems in the scope of CPM, as advocated by the SV. More generally, we would have a limitless number of denumerable families of models united by a common structure, ready-made and available for modelling the particular domain. Our only remaining task would be to go through the models and simply choose which model would best suit for the representation purpose at hand. In accordance with the SV, this is what theory-application consists of. The fact that in real scientific practice we settle with representation models that are alien to the theory-structure, or that they are not the result of only the conceptual resources of the theory, would be considered a practical matter related to the mathematical tractability and solubility of our equations. The SV instructs us that the compromise, in real scientific practice, with a model that approximately captures the features of a physical system, is of no philosophical value since a model that exactly fits exists out there in the Platonic 'world' of the structure. This is unquestionably an ideal case for science,

which I attribute particularly to van Fraassen, who is the only proponent of the SV who confines himself to an analysis of the structure of scientific theories and only on rare occasions pays attention to how theories are applied.⁵⁹ For van Fraassen is the one who explicitly holds that the class of models is assumed complete and exhaustive. Lets recall, after all, his ‘completeness’ dictum: that in one model nothing except the solar system exists, in another the fixed stars also exist, and in a third the solar system exists and dolphins are its only rational inhabitants. We thus would not be very far from accuracy if we were to assume that for van Fraassen, and more generally for the SV, scientific theorising involves the perpetual construction and use of theoretical models in the quest to complete the family of models.⁶⁰ Setting aside the different technicalities of confirmation theory, this alludes to a view on theory testing and confirmation not so far apart from that of the RV. The RV would have it that theoretical predictions can be reduced to observation statements; thus, that comparing the theory’s deductive consequences to observations can test the theory. In the same spirit, the SV sees the theory as flooded with models. The job of the scientist is to pick the right model that best represents the physical system at hand. Both of these views regard theories as constructs that in principle embody a complete and exhaustive description of their intended domains. In other words, in the case of the SV, a theory embodies all logically possible structures, which at the point of definition become available for modelling the world.

This understanding of scientific theory application conceals a number of unfounded implications. Firstly, it assumes that a sharp distinction between theoretical models and data models can be maintained. Secondly, that models constructed by pure ingredients of the theory represent phenomena (i.e. via the data models extracted

⁵⁹ Although van Fraassen has addressed issues like Glymour’s ‘bootstrapping’ method (Earman 1983) and Bayesianism (van Fraassen 1989), to my knowledge he does not address the sort of theory application that I associate with the process of model construction. I think that Suppe and Giere implicitly hold such a view too, although their versions do not require them to hold such a position.

⁶⁰ Unless we understand the SV as offered for a rational reconstruction of scientific theories, this attitude just amounts to wishful thinking, as never have we seen a theory developed by a perpetual proliferation of its theoretical models. Other authors before have made the point that in Physics we work with a restricted list of Hamiltonians, see in particular Cartwright 1983 essay 7.

from the phenomena). Thirdly, that ‘interpretation’ is something we add onto a theory once we have completed the process of theorising and we have to pay a visit to the laboratory. These are the reasons why the SV can dispense with mathematical equations (i.e. linguistic entities) and identify the theory with the structure (plus the other appurtenances -satisfaction function and elementary language, theoretical hypotheses etc.), which the equations give rise to. If the above assumptions are legitimate it then makes no apparent difference what comes first: the equations or the structure. Equations, of course, are linguistic media of representation, which the SV considers dispensable once a state-space trajectory is fixed. It could be argued that equations are simply delimitation or definition of structure, or that they satisfy an abstract mathematical structure. Nevertheless, no matter how one chooses to view the relation between equations and structure, it does not strip of equations their linguistic character. Furthermore, mathematical equations are the outcome of -frequently profound- physical reasoning that is involved in their construction, hence they are the carriers of physical content. This understanding of equations motivates the arguments that follow, and my intention is to restore the emphasis on the physical reasoning involved in setting them up. On the issue of theoretical representation, the SV shifts the emphasis to structural relations thus obscuring the physical reasoning involved in setting up equations and thus stripping equations of their physical content. It may, therefore, be useful to keep in mind a level of distinction, between equations and structure.

In Section 2.6 we saw how Hempel urges the thesis that theoretical inferences are loaded with assumptions of completeness or presupposed provisos, in order to undermine the claim that scientific theories deductively imply observation sentences. In the philosophical background of his argument, as was pointed out, Hempel targets the view that theories are deductively connected sets of statements and that some of these statements, the laws of the theory, are empirical universal generalisations; in other words, he directs his argument against the RV.

In the same set of essays, Giere (1988) addresses the problem of provisos. He argues that since the Semantic View does not consider theories as sets of statements and since the laws of the theory are only definitional devices and not empirical

generalisations in this view, Hempel's argument has an easy solution. Giere's solution to the problem is given by means of the following example. In the SV, we can construct a model that applies to a particular physical system. Suppose that the physical system of concern is a pendulum, then within CPM we can roughly characterise a model for pendulums as a system consisting of a suspended weight that has a period of oscillation, $T = 2\pi\sqrt{l/g}$. It is then, according to Giere, a theoretical hypothesis whether any particular physical system is a classical pendulum. And he continues that '[t]he minor problem that no swinging weight exactly satisfies [the preceding] equation has an easy solution. For any particular pendulum we can say that its behaviour approximates that of a classical pendulum to within specified limits on its period of oscillation.' [Giere 1988, p43] Relying on this premise, Giere goes on to conclude that the kind of problem that led Hempel to introduce provisos has an easy solution. Not all swinging weights are classical pendulums, if for example a powerful magnet, which is properly positioned in the proximity of the weight, influences the oscillations then the result is a 'magnetically augmented pendulum'. A new model has to be constructed in order to apply to this particular physical system.⁶¹

Buried in Giere's response to Hempel is the underlying position of the SV that theoretical representation of phenomena involves some output of the theory. In the case of the SV instead of observation statements, the theory outputs a model. This, however, is exactly what Hempel combats with his proviso-argument: the theory does not represent phenomena. Therefore, Giere does not offer a solution to Hempel's problem, but just an affirmation of the fact that the SV regards theories as complete and exhaustive descriptions of their domain, awaiting for the midwife to give birth to a suitable model. As I understand Hempel's problem of provisos, the only satisfactory response it can be given by the SV is that of Suppe: that the theoretical model is an abstract and idealised replica of the physical system. This statement, apparently, presupposes that representation is subject to the fulfilment of provisos. Nonetheless,

⁶¹ It can easily be argued that one can adopt Giere's claim and still reject the SV of scientific theories. It can also be argued that there is an ambiguity in the notion of 'model' as used by Giere. He talks of models united by a common state-space, yet the examples of scientific models he uses in his argument have representational attributes that go beyond the conceptual apparatus of the theory.

as pointed out in sub-section 3.2.2.4, and as it will become evident in the present and subsequent Chapters, this view is very distant from actual scientific practices and in addition, it obscures the processes of representation model construction.

In real scientific practice we are forced to stick with our mathematical and numerical methods, and in so doing we know we cannot solve -or obtain reasonable approximate solutions for- all the equations that our imagination can possibly make available. Instead, in our efforts to construct representation models we find ways to relate the insoluble equations to more tractable ones. *Prima facie*, this may seem like a purely practical issue, but after closer examination we notice that such procedures often require (significant) departures in the physics involved. As far as van Fraassen is concerned, however, this can be overlooked:

‘To say that a proposition is approximately true is to say that some other proposition, related in a certain way to the first, is true. To say that a model fits approximately is to say that some other model, related in a certain way to the first, fits exactly.’ [Van Fraassen 1985, p289]

Although I find the analogy between ‘relations of propositions’ and ‘relations of models’ ambiguous, it is on the relations of models that I want to focus.⁶² In what ways can one model be related to another, such that they both are part of a common

⁶² Clearly van Fraassen uses the word ‘proposition’ in the sense of ‘statement’. The chances of stating a word or phrase (e.g. an equation) to describe the behaviour of a physical system accurately would be, for a variety of reasons, remote. To say that our statement partially or approximately captures the behaviour of a physical system makes sense, as we are contrasting this statement to previously existing statements that were used for the same purpose, or to statements that will plausibly be asserted in the future, or to statements that describe the phenomena. Despite the fact that it may be a difficult task to explicate the relation of approximation between any of the two. In analogy however, to say that the actual model we use to describe a physical system approximates a model of our theory that exactly represents the phenomena is to contrast something that ‘exists’ with something that will never ‘exist’ (i.e. the theoretical model will never have a solution otherwise we would have used it to begin with). Hence, van Fraassen’s analogy seems flawed on two grounds. Firstly, he assigns the relation of approximation to two ‘entities’ that in principle cannot be contrasted empirically. This is a different relation to the one we mean when we say that two statements relate approximately to each other. Secondly, his position is just blind faith in the assumption that the physical concepts of the approximate model can be reduced to a handful of theoretical concepts that constitute the exact model. There is no reason why the physical components of the two models could not be different.

mathematical structure (i.e. a state-space) and it is possible to use one in representing phenomena but not the other? If van Fraassen's claim is construed narrowly then it is essentially vacuous. Most people would -with hindsight to Relativity Theory and Quantum Mechanics- agree that no Newtonian model fits the world (or a part of it) exactly. Instead, let me construe it as claiming the following:

To every model used in representing phenomena (within the scope of the theory) by virtue of its tractability, there corresponds a model in the state-space -presumably there is no difference in the features of the physical system, accounted for in the two models that stand in this relation to each other. The latter intractable model can then be said to fit the phenomena 'exactly', as opposed, and always relative, to the tractable model.⁶³

In other words, this other intractable ideal model exhausts the theoretical aptness for description. Just as the damped harmonic oscillator fits most phenomena better than the linear harmonic oscillator, so there is a model in the Platonic world of the state-space that fits the phenomena not only better but in the best possible way the particular theory of CPM could possibly allow. Similarly, although since Poincaré's attempts very little progress has been made in solving the three-body problem, and indeed any n -body problem for $n > 2$, its solution is in principle there in the state-space, and we would be able to get to it had our mathematical or numerical methods allowed it. The fact that we model the problem differently, e.g. the restricted version of the three-body problem, only shows that we try to fit a model that resembles the ideal one and fits the phenomena approximately. In this sense, van Fraassen and possibly other proponents of the SV would claim, 'approximation' requires no special attention, there is no need for a theory of verisimilitude or approximate truth. The particular circumstances dictate what constitutes a good approximation; that is, what model reasonably resembles the ideal.

To assert that the intractable exact theoretical model stands in a certain relation to the approximate or tractable model used in actual practices rests on the following two presuppositions: (1) That no new physical features are introduced in, or removed

⁶³ I believe that this is very close to what van Fraassen has in mind.

from, the latter model. (2) That representation models are at all times constructed by having some theoretical model as a basis. These are not assumptions that can justifiably be held *a priori*, however. They can only be asserted after proper investigation of the actual models used for representation purposes by scientists. And after establishing that the methods and ways by which they are constructed do not rely on the introduction of new physical features (or the subtraction of others), or on knowledge that comes from beyond the confines of the particular theory in question. And after establishing that each and every representation model is constructed by having a theoretical model as a basis. To argue against the second presupposition it is easier to use any other theory but classical mechanics as an example. I shall therefore postpone this argument until Chapter 5, where I shall try to demonstrate two distinct ways by which representation models of the nuclear structure are constructed. In this Chapter, I shall confine myself to demonstrating that representation models in CPM include physical features that go beyond the conceptual resources of the theory. Hence, that it is unjustifiable to hold that they relate approximately to theoretical models. I will therefore attempt to employ the first aspect of the twofold strategy suggested at the end of Chapter 3. Namely, that the representation models used in science do not always qualify as theoretical models.

4.1.1 The Purported Structural Description of Nature

It is important that we first re-examine the theory/experiment relation, as urged by the proponents of the SV, in order to clarify my claim that the SV rests upon a sharp distinction between theoretical models and data models. In the process, I hope to show the importance this distinction plays for the SV, as revealed by van Fraassen's attempt to argue for a structuralist representation of phenomena.

The proponents of the SV hold that, to present a scientific theory is to present a family of models. These theoretical models are to be compared with or contrasted against models of data. The data models are experimental reports that are selective and refined representations of phenomena. To construct a data model we gather the raw experimental data and transliterate them by accounting for the experimental

design and procedures, *ceteris paribus* clauses that may hold for the system of concern, the several theories of experiment, the several auxiliary theories and so forth. Once we are finished with these operations the resulting constructs are mathematical structures, which are to be contrasted against the theoretical models, i.e. other structures. This view, therefore, allows to draw the inference that ‘...the semantic approach implies a structuralist position: science’s description of its subject matter is solely of structure.’ [van Fraassen 1997, p522] Understanding science in this sense means searching for how the two structures (theoretical and data model) are related. Three kinds of structural relations are proposed in response to this quest. Van Fraassen holds that the relation is one of isomorphism or embedding, Giere that it is one of similarity, and Suppe that the theoretical is an abstract and idealised replica of the data model.

We notice that the SV implies two distinct facets to the theoretical representation of phenomena. Firstly as mentioned above, there is the nature of the structural relation between theoretical and data models. And secondly, there is the way by which the data models relate to the raw experimental data.⁶⁴ We are forced to assume, and (following the discussion in Chapter 3) I believe correctly, that the SV implies that the theoretical models are pure ingredients of the theory, and all the auxiliary theories and the entire conglomeration of background knowledge that the scientist inherits goes to constructing the data models. As was already noted in Chapter 3, the traditional account of theory application, which can be captured by the following simple schema: $T \wedge A \rightarrow E$ (where, T stands for theory, A for auxiliaries, E for empirical

⁶⁴ In the present work attention is primarily given to the first of these representation facets, not out of personal idiosyncrasy, but because the proponents of the SV have offered very little to the study of the second facet. Other than the original study by Suppes on Learning Theory and some ideas offered by Suppe (spread throughout his writings), I am not aware of any careful and detailed studies in the various sciences. It is left entirely to the reader to imagine the different situations in the different sciences that this representation facet may hold. Indeed, the arguments offered by the proponents of this view most frequently remain in the form of general conjectures. For example, because in quantum mechanical experiments we are dealing with immense numbers of data, via appropriate statistical techniques we give them some structural form the result of which, the Semanticist suggests, is in essence a data model.

evidence), is abandoned and replaced by: $A \wedge E \mapsto M_D$, $M_T \subseteq S$, and $M_D \approx M_T$. Where, M_D stands for model of data, M_T for model-type of the theory, S for the theoretical structure (i.e. class of models), \mapsto for ‘...used in the construction of...’, \subseteq for the relation of inclusion, and \approx for the relation of isomorphism, embedding or the like. This attempt to explicate the theory/experiment relation implies a sharp distinction between the pure conceptual ingredients of the theory that are used to construct the models of the theory, and every other part of our knowledge used in the construction of the data models. The inadequacy of this view to capture significant elements of science will be exposed in the duration of the rest of the Chapter.

Because theoretical representation of phenomena is, according to the SV, a purely structural relation, the SV is faced with the following problem. Assume that we have a model of a theory in which there are N distinct entities. If we choose a set of the same cardinality in the world, then (because same cardinality implies the existence of a correspondence) we have an implicit transfer of the relations in the model to that chosen set. Therefore, cardinality permitting, the world satisfies the model and consequently the theory. There is a corollary to this problem, if theoretical representation is purely a structural relation then the same theoretical model can represent two data models from entirely different domains, i.e. isomorphic data models from different sets of objects can be embedded in the same theoretical model. Van Fraassen (1997) recognises this problem and addresses it through the following example. The same exponential curve might be the shape of two distinct data models, such as, one from bacterial population growth and one from radioactive decay. But since data from radioactive samples are not relevant to a theory about bacteria, it is simply not relevant that a data model obtained in studies of radioactivity is thus structurally related to the bacterial population model. So, there may be something more to theoretical representation, besides the structure of the phenomena.⁶⁵

⁶⁵ This discussion is meant only to emphasise the importance of the distinction between theoretical and data models for the SV, by pointing to one of its uses by van Fraassen. The particular use itself leads van Fraassen, as will be seen, to a pragmatic view of model choice and theory choice. This view, which is not necessarily shared by all proponents of the SV, will be criticised in Chapters 5 and 6.

Van Fraassen allows this requisite: 'The data model is important not in itself, but in its role of representation of the phenomena. ...we must insist that this role does not consist simply in having a certain structure. The claim of adequacy [of a theory to the phenomena] is with respect to the structure of real phenomena described in terms of the relevant parameters of the theory.' [*Ibid.*, p524] So the challenge to structuralism, he continues, is '...that the theory does not confront the observable phenomena, ...in and by themselves, but only certain descriptions of them.' [*Ibid.*, p524] This problem may induce the temptation to think that there must be something about the phenomena, besides their structure, that matters to theory representation. For example, we could think that there must be a naturally privileged description of the phenomena, to which we are forced to attribute special ontological status. But van Fraassen disputes this requirement. He dismisses this challenge by claiming that for the users of the theory (i.e. us) the claims, 'the theory is adequate to the phenomena' and 'the theory is adequate to the phenomena as described', are pragmatically the same, therefore nothing ontological follows. So, he asks, what is it about the data model, other than its structure, that makes it important to a scientific theory?

'We must add, using our own language, that for example this data model summarises certain findings about bacteria, or about radioactive decay, as the case may be. Because representation is something we do, and not something that exists in nature independently of what we do, our claim of adequacy for the theory must involve reference to how we are using both models -data model and theoretical model- to represent our subject matter.' [*Ibid.*, p525]

Van Fraassen's argument appeals to pragmatics by relying primarily on two premises. On the one hand, the division of scientific models into models of the theory and models of data; and on the other hand, on the claim that representation must involve reference to how we use these models to describe and represent the world. Do these premises reflect actual scientific theorising? In the course of this Chapter, both of these will be challenged. The theoretical representation of phenomena by means of the division of models into 'theoretical' and 'data' models, as the SV suggests, is in fact a simplification; actual practices indicate a much more complex theory/experiment relation. In my discussion of the pendulum, I shall argue that to turn the linear harmonic oscillator model into a representation model of the pendulum, we must blend it with experimentally determined parameters. The case of the pendulum is a clear indication that the linear harmonic oscillator represents

nothing in the real world, and in order to turn it into a representation device we must go beyond the conceptual resources of the theory. This goes to show that in actual science the theoretical structures are not contrasted to data models. The *terminus a quo* of a representation model, may be (in a large number of cases) a theoretical model, but only in physics books are theoretical models ‘contrasted’ to phenomena. When we want to construct a theoretical representation of the real world, the situation is not so neat and orderly, as we shall see in Section 4.3.

The question of *how we use* the theoretical and data models to represent is, in my view, derived from the question of *what* we employ in our models to turn them into representations and *how* we determine the features that are to be included in our representation models. There seems to be two underlying intuitions to van Fraassen’s claim. Firstly, we as the agents of using structures to represent phenomena fix the relation between structure and nature and thus single out on pragmatic grounds the theoretical model to be used for representation. This view seems at odds with scientific practices. It is not we alone who single out the structure but we through our interaction with the world, e.g. through our experimentation. In the discussion of the pendulum, we shall see that a number of empirical laws and experimentally determined parameters are employed to construct the representation model. The choice of these laws and parameters determines the particular representation model as opposed to other isomorphic structures. To say that these parameters are chosen on pragmatic grounds is to ignore our interaction with the world. This point, of course, only goes to indicate that van Fraassen regards theoretical models as constructs that possess the capacity for representing phenomena, whereas my claim is that representation models are devices that are partially independent from theory. Van Fraassen’s second intuition is that the subject matter of a theoretical model is fixed at the point of comparison to the appropriate data model. This understanding seems to me consistent with the view analysed earlier, that models are simply picked out from the ‘womb’ of the theory and offered for the potential representation of a physical system. This intuition will also be challenged in the course of this and consequent Chapters. In fact, my thesis is that the theory is always understood as ‘what it is a theory of’, i.e. what the terms of the model equations denote, and that this is manifested at every stage *en route* to constructing representation models.

4.2 Using Models to Represent Physical Systems

4.2.1 A Useful Distinction: The *Ideal* vs. the *Concrete* Model

To analyse van Fraassen's 'approximation relation' amongst models, we must look into actual constructions of representation models. As in this Chapter I will be restricting myself to CPM, it is useful to propose a distinction by which to work. By virtue of the nature of their relation to theory, there are two ways by which to view scientific models that are meant to represent phenomena. Let me call the first kind the *ideal* model (model_I) and the second kind the *concrete* model (model_C).⁶⁶ Let us assume that both of these kinds of models can function as representations of phenomena. Hence, in order for the two classes of models to be considered disjoint, the distinction must be understood with respect to *what conceptual resources are used* for the construction of models from each class in representing phenomena. The model_I should be understood as a construct in which only the conceptual resources of the theory are utilised. By contrast, I wish to understand the model_C as a construct in which much else besides the conceptual resources of the theory are utilised.

Let us examine the distinction more closely. The ideal model is derivative of the theory, whether it is viewed as an interpretation of the language or as a directly definable mathematical structure consisting of a set of mathematical objects and relations they enter into (as examined in Section 3.3), is of no consequence to the argument. In CPM ideal models are those for which there is *definite* knowledge of the system-Hamiltonian, and hence for which points in phase space correspond to *definite* states of the system. Those models that qualify as members of the model_I class we usually cannot articulate in any mathematically useful way, but (most frequently) only with intractable sets of equations consisting of unspecified parameters. In fact

⁶⁶ This distinction is only meant to facilitate my analysis for models of CPM and not to hold universally. In fact, it will become clear in Chapter 5 that for nuclear models this distinction is useless.

only a handful of them are theoretically useful and they are what Giere recognises as being exemplars in a Kuhnian sense, i.e. models upon which other models can be modelled, such as the Linear Harmonic Oscillator or Orbits in an Inverse Square Force Field. Viewed from this perspective, the class of theoretical models, advocated by the proponents of the SV, is the class of ideal models, hence each theoretical model is a model_I. Now, I am willing to admit that these models have the capacity of representing phenomena, at least in CPM. But, I shall try to argue that it is an obscuring simplification to regard the actual models used in science for representing phenomena as models in this sense. To put it differently, it is unjustifiable and misleading to identify the models physics students are taught in standard textbooks with the actual models scientists construct to explain the phenomena and make predictions. What are concealed or obscured, by such views, are the complexities in methods and processes involved in modelling phenomena, as well as the nature of the resulting construction. Underlying such views is, of course, the conviction that pure theory is or can be the only source for the construction of representation models.

It is true that in many cases models, that can be said to belong to the model_I class, are used to model and represent physical systems. In some of these cases the resulting equations of motion are analytic and with exact solutions. Such would be the case for the isolated mass-spring system, which with the proper idealising assumptions we can to a satisfactory degree model in terms of the equation of motion for a linear harmonic oscillator. In other cases the ideal model does not result in analytic equations, yet approximate solutions to such equations are available and the use of the particular model_I is advanced (such is the case when the equations give rise to some elliptic integrals). In the latter case, we have what Redhead (1980) suggested as one of the two possible points of view of approximation, namely approximate solutions to exact equations. Consider his example:

‘For the equation $dy/dx - \lambda y = 0$ we might expand our solution as a perturbation series in λ , the n th order approximation being just

$$y_n = 1 + \lambda x + \lambda^2 x^2 / 2! + \dots + \lambda^{n-1} x^{n-1} / (n-1)!$$

if we consider the boundary condition $y=1$ at $x=0$.’ [*Ibid.* p150]

The second view of approximation that Redhead suggests is when we look for exact solutions to approximate or simplified equations. In the example above, y_n is an exact solution to the equation $dy/dx - \lambda y + \lambda^n x^{n-1}/(n-1)! = 0$, which for small λ is approximately the same as the original equation above.⁶⁷ Despite the fact that ideal models may be used in theoretical representation of phenomena by the use of approximation techniques, a model_I can only represent isolated physical systems, under experimental conditions that do not seem to hold outside of the laboratory. In order to convert the model_I into a representation of the real world, or some part of it, we must introduce additional features to it.

Concrete models contain a set of equations that may be soluble either by exact or approximate available methods, but since this may also be true about a large number of ideal models the characteristic of mathematical tractability is not the place where to search for the distinction. The distinction I want to promote between model_I and model_C is not one based on tractability, but one based on the physical reasoning involved in our model construction. In using models from the model_I class to model physical phenomena we utilise the theory and its conceptual apparatus in a direct way. When such a procedure results in a model with an insoluble set of equations and simultaneously with no contribution to our physical insight,⁶⁸ then the theory - construed narrowly as a class of models- is useless on its own. We have to look at the problem from outside the 'theory-prism' in order to construct a model_C that captures in concrete ways the features of a physical system. In its generality, this is not a claim that can be convincingly defended in the case of CPM. For the class of theoretical models to be useless in the construction of representation models, we must encounter modelling cases where theoretical models play no role in the heuristic. I shall argue that we encounter such cases in modelling features of the nucleus, but this argument has to wait until Chapter 5. Most frequently in CPM this is not the case. Indeed, the

⁶⁷ As Redhead stresses '...the two approaches are equivalent in the sense that if we consider an approximate solution y_n for an exact [equation] ...we can always specify [another equation] ...which is 'approximately' the same as the first, for which y_n is an exact solution.' [*Ibid.* p150]

⁶⁸ Recall that very often much of our physical insight into the behaviour of real systems comes from an analysis of the *solutions* to the equations of the model that purportedly represents the system.

most frequent method of construction of concrete models that we encounter in CPM has an ideal model as a starting point. However, the resulting model_C departs in significant ways from the model_I employed at the outset. The primary reason for this, as I shall be arguing, is that the concrete factors added to the model_I to turn it into a representation of the physical system, do not originate from the conceptual apparatus of the theory.

It may seem that by restricting myself, at this stage, to CPM I am making the case for the SV. To use Quine's phrase CPM is a scientific 'limit myth', and as such it offers itself as a paradigm case to the Semanticist, and in particular to Suppe's understanding of the state-space, for two reasons. Firstly, all models used for representation purposes (whether they belong to the model_I or model_C class) are, in one way or another, related to the theory structure. To put it in the jargon of the SV, the models that do the representation of the phenomena within the scope of CPM are either the theoretical models that comprise the theory, or models that use the former as a basis in their construction. In both cases, it may be argued (although I explicitly express my doubts about this), the theoretical constituents of the models are pure ingredients of the theory of CPM. Secondly, Newton's laws hold only for inertial reference frames. We know that the species of inertial systems is fictional. In fact, inertial reference frames can be regarded only as idealisations or approximations of actual non-inertial systems. For example, to describe the motion of a particle relative to a body that is rotating with respect to an inertial frame would clearly be a complicated matter. However, the problem can be made relatively easy by introducing non-inertial forces as correction factors to Newton's 2nd law. These forces are usually referred to as the *centrifugal* and *Coriolis* forces. They are not forces in the usual sense of the word; they are introduced in an artificial manner as a result of the arbitrary requirement that we be able to write down an equation that is valid in a non-inertial reference frame and that resembles Newton's equation. When these forces are introduced into Newton's equation, for a particle in a non-inertial frame experiencing an effective force F_{eff} , then the resulting equation looks as follows: $F_{eff} = ma_r = ma_f - m\omega \times (\omega \times r) - 2m\omega \times v_r$. The second term is the centrifugal force

and the third is the Coriolis force.⁶⁹ To put this point in terms of the jargon of the SV, we do not introduce another set of models to deal with representations that are more realistic. Instead, by introducing ‘fictional’ correcting terms we improve on the equations defined by Newton’s laws, i.e. we do this because of the arbitrary requirement to construct models that belong to the ‘family’. Yet despite all this, it is because CPM is a ‘limit myth’ that we can use it to reveal those aspects of physical reasoning that facilitate our theoretical representation of phenomena, and that go beyond the narrow construal of the theory as a class of abstract structures. So my attempt in this Chapter is in the same spirit as most philosophy of science. Proceeding under the assumption that CPM is a paradigm example of a scientific theory, if it can be shown (even partially) that the SV fails to account for some of its significant features then it can be shown that it is an inadequate view of scientific theories.

Despite the fact that in CPM most of the modelling of phenomena is done by using a $model_I$ as basis for the representation model construction, if we focus our attention on the methods of construction we can still discern two distinct ways by which the construction is achieved. These two distinguishable general procedural-modes of construction of a $model_C$ for CPM systems can be instructive. The first mode directs us to see how the resulting $model_C$ can be used for representing a real system. By utilising a $model_I$, initially we may disregard some of the features of the physical system; as is the simple case when the equation of motion of the two-dimensional oscillating pendulum, which does not generally have exact solutions (as described briefly in Chapter 3), with proper assumptions is reduced to that of a linear harmonic oscillator. In such cases, we do not model the actual physical system but some features of it, and the modelling is done by the use of other familiar models with known solutions. But in order to use the model to represent something real, *inter alia* we have to reintroduce the abstracted features, as is the case when the pendulum is used to compute the Earth’s acceleration due to gravity. The ways and procedures by which these features are ‘reassembled’ are what make the difference between the ideal and the concrete models. In order to establish the mathematical representation

⁶⁹ See Goldstein 1980, and in particular chapter 4, for an elaborate account of the mathematical formalism leading to this equation.

of these features we need to employ an assemblage of empirical laws and experimentally determined parameters. Hence, it may be argued that the influencing factors present in the concrete model are not derivatives of the theory. I shall present the case of modelling the pendulum as an example of this sort.

The second mode of construction is a totally theory-dependent method. This being the case, we would expect that it would be the best case scenario for the understanding of scientific theories given by the SV. Yet, my examination yields quite the opposite inference. This method directs us to see how specific adjustments to the general model_I can be made in order to learn about the *physical content* of the theory. Via this method, we modify the *physical* problem (sometimes in radical ways), not so that we can fit it into the theory, but to study specific aspects of the model in question. This can sometimes be done by introducing further idealisations, thus distorting the physical situation even further. The result is not proposed for an adequate representation (although it can be viewed as such), rather it is primarily used to gain some insight into the physical properties of the system. In doing so, the physical reasoning involved in our practices indicates that our view of what theories are is intertwined with what the theory is about, i.e. what the elements of our equations denote. This cannot be captured by a rational reconstruction of theories as classes of models, where the representation relation is reduced to a relation of structures. An example that falls into this category is the ‘restricted version of the three body problem’, which will be examined in detail later.

Concrete models are the result of these general modes of construction. They are models that come about by using ideal models as their basis (at least, in CPM). But the physical reasoning involved in constructing them is essential to their use in representation. Investigating how model_C relates to model_I will help us see whether the representation relations suggested by the proponents of the SV have any gravity or, indeed, any legitimacy at all. In the next chapter, I shall extend my argument to claim that in many cases in modern physics we cannot convincingly defend the position that a model_C is constructed solely by means of having a model_I as a starting point. In fact, I shall argue that in constructing concrete models that can be used to represent nuclear phenomena, our physical reasoning derives from the conceptual

apparatus of more than one of our theories. This process sometimes leads to representation models with conflicting features. Furthermore, I shall argue that in looking at the concrete models in a historical dimension we detect an influence on each by its predecessors.

4.2.2 ‘Theory Entry’

Before beginning to look into the relation of model_C to model_I , we must first look into how physical systems are modelled. Cartwright (1983) has dubbed this process ‘theory entry’. Despite the fact that her discussion is couched in the parlance of the logical positivists of bridge principles and internal principles, what is important here is the kernel of her argument, which is how we fit facts to equations. The ‘fitting of facts to equations’, she suggests, is a process that can be divided into two stages. As a first stage we prepare an informal description of the phenomenon such as to ‘...present the phenomenon in a way that will bring it into the theory.’ [Cartwright 1983, p133] In this stage we use our background knowledge and try to confine the description to those elements that will allow us to match an equation to the behaviour of the physical system. But this is not enough, for not any equation is soluble. In fact we need to know in advance what boundary conditions and what approximation methods can be used in the solution of the resulting equations. In the second stage, we look at the description through the prism of the theory and dictate the necessary equations, boundary conditions and approximation methods.⁷⁰

Let me try to demonstrate this process by means of a simple example from Classical Mechanics that we can fit into an ideal model without exerting ourselves (a familiar example that can be found in most books on Partial Differential Equations or Mathematical Physics). Imagine a flexible stretched string in a horizontal orientation (along the x -axis), of length L and mass per unit length μ , attached on the left side to a

⁷⁰ Of course, Cartwright intends this view as a simplified and idealised picture of what actually takes place, and so it is intended here.

fixed support and on the right side to a tightening screw. This is very much like the situation we encounter in various stringed musical instruments. The string experiences a *tension* T along the x -axis by the supports at each end. T is defined as the magnitude of the force exerted by the two supports on the string, so the net force on the horizontal portion of the string is zero. The force will generally stretch the string a bit, and possibly deform the support at the left end. We assume however that the supporting system is rigid and that nearly all of the deformation occurs in the string.

If the stretching is not excessive then Hooke's law can be applied to the system, so that the initial tension in the string is taken to be equal to some force constant multiplied by the length of stretch. Now suppose the string is stretched further, say by plucking it. Or consider an arbitrary deformation of the string, so that its transverse displacement in the y -direction at a distance x from the left end is $y(x)$. The extra stretching associated with this deformation is equal to the arc length of the string minus the initial length:

$$\int_0^L \sqrt{1 + [\dot{y}(x)]^2} dx - L \quad (4.1)$$

When the displacement $y(x)$ is small compared to L and there is a sufficiently smooth variation so that the slope $\dot{y}(x)$ is small compared to one, the square root in the above integral can be approximated by retaining only the first two terms of a binomial expansion, yielding an approximated stretching:

$$\int_0^L [1 + \frac{1}{2}\dot{y}^2] dx - L = \frac{1}{2} \int_0^L [\dot{y}(x)]^2 dx \quad (4.2)$$

The tension will be equal to the force constant times the total stretch, which will be the initial stretch plus the second order quantity given by equation (4.2). Since equation (4.2) tells us that the increase in length is quadratic in the relative amplitude of the deformation, the extra stretching will be small compared to the amount of initial stretch given to the string. Thus in a first-order approximation model it can be neglected. This is another way of saying that we may assume the tension to change insignificantly when the string is plucked. All the preceding reasoning is done with the sole intention of justifying the assumption that the tension on a vibrating string is

approximately constant. This will eventually allow us to fit the description of the system to the intended equation of motion.

To get an equation of motion for the string we employ Newton's 2nd law, which states that the acceleration of an infinitesimally small segment of the string is equal to the net force on the segment. We focus on a small arbitrary segment of the string between x and $x+\Delta x$. The vertical displacement at time t can be represented as a function of two variables $y(x,t)$. The mass of the segment is $\mu\Delta x$. Thus the y -component of the net force on the segment can be written as:

$$\mu\Delta x \frac{\partial^2 y}{\partial t^2} \quad (4.3)$$

Now it can easily be seen that the transverse force exerted on the segment by the neighbouring segments of string depends on the slope of the string at the ends of the segment. Let θ_1 and θ_2 be the angles between the string and the horizontal axis at the left and right ends of the segment respectively, then:

$$\tan \theta_1 = \left(\frac{\partial y}{\partial x} \right)_x \quad \text{and} \quad \tan \theta_2 = \left(\frac{\partial y}{\partial x} \right)_{x+\Delta x} \quad (4.4)$$

The y -component of the force on the segment due to the adjacent string parts is just $T\sin\theta_2 - T\sin\theta_1$. For small angles, consistent with the first-order calculation used here, the difference between $\sin\theta$ and $\tan\theta$ is neglected. If we also neglect other forces, such as the weight of the segment and friction on the segment, Newton's 2nd law can now be written as:

$$\mu\Delta x \frac{\partial^2 y}{\partial t^2} = T(\tan \theta_2 - \tan \theta_1) = T \left[\left(\frac{\partial y}{\partial x} \right)_{x+\Delta x} - \left(\frac{\partial y}{\partial x} \right)_x \right] \quad (4.5)$$

If this equation is divided through by $\mu\Delta x$ then in the limit as Δx turns to zero we have:

$$\frac{\partial^2 y}{\partial t^2} = \frac{T}{\mu} \frac{\partial^2 y}{\partial x^2} \quad (4.6)$$

It is important to note that in writing down Newton's 2nd law we have ignored the term due to the external forces. We could have grouped together all other forces acting on the segment into one function, by letting $F(x,t)$ be the y -component of the sum of all external forces acting on the string per unit length in the x -direction. In

more accurate contexts, $F(x,t)$ must be included in both of the following equations (4.7) and (4.8), for it is part of Newton's law.⁷¹ On dimensional grounds, the quantity T/μ must have the units of the square of speed. It turns out that this quantity is equal to the square of the speed of waves on the string. Hence, defining $c = \sqrt{T/\mu}$ we can write equation (4.6) in the usual form of the *scalar wave equation*:

$$\frac{1}{c^2} \frac{\partial^2 y}{\partial t^2} = \frac{\partial^2 y}{\partial x^2} \quad (4.7)$$

The scalar wave equation was the model intended from the start. It has the benefit of having a well-known general solution. To reach the point of fitting the physical system to the equation we had to go through a process of physical reasoning which involved the justification of several assumptions such as the constancy of the tension. Of course, the idealising assumption about the constancy of the tension could be avoided, in which case in the limit of equation (4.5) as Δx turns to zero T would be considered a differentiable variable and the result would be the *homogeneous Sturm-Liouville* equation of motion:

$$\frac{\partial}{\partial x} \left(T \frac{\partial y}{\partial x} \right) - \mu \frac{\partial^2 y}{\partial t^2} = 0 \quad (4.8)$$

Only for certain particular forms of the functions T and μ can solutions to this equation be obtained in terms of simple functions. For arbitrary functions T and μ there is no general method of solution to the homogeneous Sturm-Liouville equation.⁷²

The preceding example may seem, of course, a case that demonstrates the heuristic advantages of the SV. It shows how a physical system can be fitted into one of the theoretical models (exemplars) of the theory. But I have chosen such an example only to demonstrate the process of theory-entry, and not to claim that this is the only way

⁷¹ For example, if the external force acting on the string is gravity then $F(x,t) = \mu g$; other possibilities include a damping force then $F(x,t) = -\beta v_y$, where v_y is the velocity in the y -direction; or a linear restoring force in the y -direction, $F(x,t) = -ky$.

⁷² In various areas of mathematical physics, particularly in quantum mechanical problems, this type of equation frequently occurs in its inhomogeneous form, with the right-hand side not equal to zero.

by which this process is achieved.⁷³ The main issue in this example (which of course is not offered as a criticism of the SV) is that, in attempting to construct a theoretical description of a physical system we must either distort the actual picture (as in the case of justifying the constancy of the tension) or totally ignore factors that may influence the phenomenon (as in the case of air damping). This is the reason why Cartwright claims that ‘...the ‘right kind of description’ for assigning an equation is seldom, if ever, a ‘true description’ of the phenomenon studied...’ [Cartwright 1983, p133] And this is not all. We can also discern that there is a wealth of physics in modelling a physical system. Beginning with the unprepared description through to the fitting of the equations, the procedure is not just one of matching a mathematical structure to the physical system at hand. More generally, we could say that to define force functions, or Hamiltonians, we need physical descriptions of the physical systems and mechanisms involved in their construction. It is these descriptions that explain the choice of the force functions. Indeed, our descriptions of the physical systems aim to fit them into one of the models in the available list of force functions (or Hamiltonians). By the mere fact that the list is confined to a few (exemplar) models, to expect that our resulting models represent the set of objects of the physical system and the relations they enter into is, to say the least, a simplification.

Of equal importance is another role that these descriptions have. They tell us what factors have been omitted from the force function and they act as guidelines to what corrections we must make in order to match theory to experiment. I take this to be the key issue in the context of my discussion; the idea of theory entry becomes important in theory-application because it opens up the scene for a third stage, that of theoretical representation. This involves the ‘moulding’ of the equations as to capture as many of the physical features of the system as our knowledge and limitations allow. There is no -independent from experience- reasons why these corrections should be confined to the conceptual resources of the theory, as the following example of the pendulum indicates. The general question, to be explored next is what happens when we attempt to model real physical systems, by removing the idealising

⁷³ A different kind of theory-entry can be discerned for the example of the *liquid drop model* of the nucleus, which is examined in length in Chapter 5.

assumptions and introducing into the external force function those factors that were initially omitted in the process of theory-entry. Focussing our attention to this aspect of modelling, we discern that representation models (of the model_C class) do indicate signs of theory independence.

4.3 The Simple Pendulum: Measuring g

The straightforward model of the simple pendulum has been discussed by Morrison, in an attempt to show that despite ‘...close links with theory there is an important sense in which we can see the pendulum model functioning independently, as an instrument that is more than simply a vehicle for theory.’ [1999, p48] The core of her argument is that in order for an accurate theoretical model as this to accurately represent the respective physical system, we must add several correction factors. The addition of the correction factors contributes to the model’s independence from theory because it makes it more concrete and allows it to function as an entity in its own right.⁷⁴ I wish to add to the argument that the model functions as an independent entity because the correction factors are introduced by physical considerations that transcend the theory’s conceptual apparatus.

Let us assume for the sake of brevity that ‘theory-entry’ for the pendulum has been achieved by a description of the motion that has the following main characteristics: a mass-point bob supported by a massless inextensible cord of length l oscillates about an equilibrium point. The equation of motion of such a system was already given in chapter 3 and noted that it gives rise to elliptic integrals (ignoring, as noted above, the term due to external forces):

$$\ddot{\theta} + \frac{g}{l} \sin \theta = 0 \quad (4.9)$$

⁷⁴ I consider my discussion of the pendulum as complementing hers in some ways. Although the conclusions drawn are certainly not identical, this is primarily due to the fact that I expand the argument in a different direction and use it for a different purpose.

If infinitesimal displacements are assumed, then it can be reduced to the equation of motion of the linear harmonic oscillator, which is the starting point:

$$\ddot{\theta} + \frac{g}{l} \theta = 0 \quad (4.10)$$

The solution of this equation yields a relation among the period T_o , the cord length l and the acceleration g due to the Earth's gravity. Knowledge of the cord length and the period will allow us to solve for the acceleration of gravity:

$$g = 4\pi^2 l / T_o^2 \quad (4.11)$$

The experimental problem of determining g , therefore comes down to measuring l and T_o . However, T_o is far from an acceptable range of accuracy to the experimental value of the period T , since it is known that the actual pendulum apparatus is subject to influences (some of which belong to the omitted external force function) that are not accounted for in the idealised assumptions underlying equation (4.10). Nelson and Olsson (1986) give a known, but not necessarily exhaustible, list of influencing factors: (i) finite amplitude, (ii) finite radius of bob, (iii) mass of ring, (iv) mass of cap, (v) mass of cap screw, (vi) mass of wire, (vii) flexibility of wire, (viii) rotation of bob, (ix) double pendulum, (x) buoyancy, (xi) linear damping, (xii) quadratic damping, (xiii) decay of finite amplitude, (xiv) added mass, (xv) stretching of wire, (xvi) motion of support. These influencing factors, some of which increase and others decrease the period, can be grouped into four categories based on their causal origin: the finite amplitude correction, the mass distribution corrections, the air corrections, and the elastic corrections. Nelson and Olsson proceed to show how the value T_o can be corrected by introducing the different correction factors into the equation of motion.⁷⁵

Now, ideally what would be expected here is for all of these influencing factors and their mutual interactions to be included in an appropriate mathematical expression, using the conceptual resources of the theory. This would result in a model close to the ideal. We could then follow van Fraassen and speak of the relation between this fictional model and the ideal as one of pure approximation. Approximation in this

⁷⁵ Nelson and Olsson 1986, Nelson 1981, and Olsson 1976 offer a thorough and elaborate analysis of the pendulum theory/experiment match.

case would be introduced in a mathematical context and it would be of a pragmatic nature; we would have inserted into the approximate model all the factors that influence the physical system, and their mutual interactions, and solved the resulting equations by some appropriate (perturbation) method. The relation between the fictional model and the ideal model would require no further scrutiny. No matter how appealing it may seem, this is, however, a fictional scenario with the self-evident ending: the model equations are unattainable.

What takes place instead, which is not peculiar to the pendulum, is that the mathematical functions, for each influencing factor, (because their corrections to the period are small compared to T_0 , they are assumed to add linearly) are inserted into the equation of motion (4.10) in a cumulative manner. Employing the principles of superposition for differential equations, we are left with a system of linearly independent equations, each with a different influencing factor. From the solutions, the total value of the correction is computed by adding all the effects linearly. So far in this story, the SV seems to work well but there are two interconnected problems that we should not undervalue. Firstly, if it is assumed that some of the linearly independent equations are solved by some appropriate approximation method (which is what actually takes place), then following Redhead's suggestion of viewing approximations, it is the case that different approximations will yield somewhat different force functions. Many of these are not experimentally distinguishable. Thus, we have no non-arbitrary way of singling out the preferred model_I even in principle.⁷⁶ Hence, Van Fraassen's *a priori* assumption of the relation between model_C and model_I (being one of approximation) is unfounded. In addition, as we shall see, since these force functions are usually determined by the employment of empirical laws (and empirically determined parameters), the status of the latter is operative in the construction of model_C.

⁷⁶ The proponent of the SV is left with the option of claiming that the model is singled out purely on pragmatic grounds. This line, however, seems to ignore that the concrete model postulates mechanisms at work in the physical system. I shall attempt to argue against this kind of response in Chapter 5.

Secondly, it is not just the mathematical constraints that force us to integrate the individual effects, rather than combine the causes and their mutual interactions into the external force function and simultaneously remove any idealising assumptions. What is at stake in this problem, i.e. filling in for the external force function and removing the idealising assumptions, is not necessarily a straightforward process. It is one which is performed by the employment of a variety of theories and of our knowledge from disparate areas of physics, in an effort to *extend the domain of application* of the theory beyond the scope of the class of its theoretical models.

Exempli gratia, we could examine the corrections due to the finite amplitude, the finite radius, the buoyancy, the air damping and the string stretching. The correction due to the finite amplitude is made by solving equation (4.9). Solving this by a perturbation expansion gives us the following correction for the period:

$$\Delta T = T_o \sum_{n=1}^{\infty} \left(\frac{(2n)!}{2^{2n}(n!)^2} \right)^2 \sin^{2n}(\theta_o/2) \quad \text{where, } \theta_o \text{ is the maximum angular displacement.}$$

A real pendulum has a bob of finite size, a suspension wire of finite mass and in addition, the wire connections to the bob and the support have structure. All these factors have some contribution to the oscillations. Their effects are incorporated into the physical pendulum equation: $T = 2\pi\sqrt{I/Mgh}$. Where, I is the total moment of inertia about the axis of rotation, M is the total mass and h is the distance between the axis and the center of mass. Depending on the shape of the bob we could calculate its moment of inertia and thus compute its contribution to the period of oscillation. If we assume that the bob is a perfect sphere of radius a then, $I_{bob} = ml^2[1 + \frac{2}{5}(a/l)^2]$, $M = m$, and $h = l$. Using this information for the rigid body mechanics of a perfect sphere, we compute a theoretical correction to the period, $\Delta T = \frac{1}{5}(a/l)^2 T_o$. Knowledge of l and a can thus be used to compute the correction value. In a similar manner the correction contributions due to the wire connections and the mass and flexibility of the wire can be computed.

Since the pendulum experiment takes place in air, it is expected that by Archimedes' principle the weight of the bob will be reduced by the weight of the displaced air. Since under such circumstances the effective gravity is reduced, this increases the period. The correction is $\Delta T = \frac{1}{2}(m_a/m)T_o$ where m_a is the mass of the displaced air. In addition, the air resistance acts on the oscillating system (pendulum bob and wire) to cause the amplitude to decrease with time and to increase the period. Initially, we started with a law of force and used it to find a model for the description of the physical system. The reverse general problem of finding the law of force that may be responsible for a particular correction factor of the starting model is of equal importance. The Reynolds number for each component of the system determines the law of force for that component: $R = \rho VL/\eta$ where, ρ and η are the fluid density and viscosity respectively, and V and L are characteristic values of velocity and length. The drag force is usually expressed in terms of a dimensionless drag coefficient C_D , which is a function of R : $F = \frac{1}{2}C_D A \rho v^2$. For values of $R \leq 1$, the force is proportional to the velocity; and for values of $10^3 \leq R \leq 10^5$, the force is proportional to the square of the velocity. It can be shown that a quadratic force law should apply for the pendulum bob, whereas a linear force law should apply for the pendulum wire. Hence, it makes sense to establish a damping force which is a combination of both linear and quadratic velocity terms (b and c are physical damping constants):

$$F = b|v| + cv^2 \quad (4.12)$$

To determine these physical damping constants we, employ the work-energy theorem, assume an appropriate velocity function $v=f(\theta_o, t)$, assume conservation of energy and match them to experimental results. We proceed to solve the equation of motion. Since the effects of both damping forces are small we can regard them as independent perturbations and set up the following linearly independent equations of motion:

$$\ddot{\theta} + (b/m)\dot{\theta} + (g/l)\theta = 0 \quad (4.13)$$

$$\ddot{\theta} - (cl/m)\dot{\theta}^2 + (g/l)\theta = 0 \quad (4.14)$$

Equation (4.13), which represents the linear damping, can be solved analytically to yield a correction factor: $\Delta T = \frac{1}{2}(b/2m)^2(g/l)T_o$. Equation (4.14), however, can neither be solved exactly, nor is it analytic, since the sign of the force must be

adjusted each half period to correspond to a retarding force. By an appropriate perturbation expansion, we obtain the correction factor: $\Delta T = \frac{1}{6}(cl/m)^2 \theta_o^2 T_o$.

The length of the pendulum is increased by stretching of the wire due to the weight of the bob. By Hooke's law, when the pendulum is suspended in a static position the increase is $\Delta l = mg/k = mgl_o/ES$, where S is the cross-sectional area and E is the elastic (or Young's) modulus. The dynamic stretching when the pendulum is oscillating is due to the apparent centrifugal and Coriolis forces acting on the bob during the motion. We model this feature by using the spring-pendulum system to the near stiff limit. The result is the following coupled equations of motion:

$$(1+\xi)\ddot{\theta} + 2\dot{\theta}\dot{\xi} + \omega_p^2\theta = 0 \quad (4.15)$$

$$\ddot{\xi} + \omega_s^2\xi - \dot{\theta}^2 + \frac{1}{2}\omega_p^2\theta^2 = 0 \quad (4.16)$$

Where, θ is the deflection angle, ξ is the fractional string extension, l is the dynamic and z_o the static pendulum length, and $z_o = l_o + mg/k$, $l = z_o(1 + \xi)$. And where, $\omega_p = \sqrt{g/z_o}$ is the pendulum frequency and $\omega_s = \sqrt{k/m}$ is the spring (string) frequency. Solving this system of equations yields a correction factor for the period: $\Delta T = \frac{1}{16}(mg/ES)\theta_o^2$.

I think we can see that the processes involved in turning an ideal model into a concrete one are diverse and hide a number of complexities. Once the model has been chosen, it is necessary to construct the mathematical apparatus to find a solution. One such example is, equations for finding the values of experimental observables. These equations usually contain some parameters (e.g. the physical parameters in equation (4.12)), the values of which are determined by comparison to experiment. Because of these adjustable parameters, agreement with experiment is not what guarantees the accuracy of the model. Frequently, entirely different models, each with its own parameters, provide equally good and plausible descriptions of a physical system. Another feature of the construction, which is obvious in the pendulum, is the employment of different empirical laws for determining the various force laws used in the concrete model. Archimedes' principle, the Reynolds number and the drag force expression, and Hooke's law are such examples. These laws (as well as the

physical parameters), which for the case of CPM may be antecedently available, have no connection, in a deductive sense, to the theory. The RV would have accommodated them into its correspondence rules or bridge principles; the SV however has no other place but the models of data, into which to accommodate them. This would, of course, be a distortion of the picture. What we are faced with is a blending of experimental parameters and empirically determined laws together with a model of the theory to produce a model_C. The theoretical model is a pure derivative of the theory and in order to turn it into a representation of a real system we must blend it with these ingredients.⁷⁷ All this can be leapfrogged by the Semanticist, who could respond that this resulting representation model is a model in the theoretical structure, where all the experimentally determined parameters are just parameters of the model, adjusted in various ways as to match the theoretical prediction to the experimental measurement. But we have seen at the very start that there is no non-arbitrary way of singling out this model if the methods employed in the construction of model_C are neglected. Adopting an analysis of scientific theories that undervalues the methods by which a model_C is constructed, is, to say the least, a mere reconstruction, that seems to lead some proponents of the SV to a complete denial of the ontological commitments of the theory and of the (auxiliary) empirical laws.

Although the SV could be made to work as a rational reconstruction, so far as the example presented here and I believe for most CPM models,⁷⁸ it does so by distorting the meaning of the *external force function* F in Newton's 2nd law. This function is treated as the unspecified part of the law, so it is assumed that anything can be substituted for it and thus that it gives rise to an indefinite number of nested models. But F is that part of the law which is about the real system of concern. The law can be used to define classes of structures, but once the ideal structure is chosen we need to examine the real system to fill in for F . Because, F is about how the behaviour of an assumed ideally isolated system is affected when it is allowed to interact with real

⁷⁷ Of course, for the theoretical model to say anything about the world it must be supplemented with initial and boundary conditions, a question that I have chosen to ignore. I believe that an argument on similar grounds can be given about the introduction of such conditions into the theoretical model.

⁷⁸ Because as mentioned earlier, in CPM we frequently use a model_I as a basis for the construction.

things (or be part of a system of real things). In order to capture this ‘change’ in the behaviour we cannot just ‘fiddle around with’ mathematical functions until we find the one that works; we must do that by *observing* and *manipulating* the specific real system.

Moreover, once we have chosen a starting model we are faced with the task of relaxing some of the idealisations involved in its construction. I say ‘relaxing’ because a complete removal is a limiting case. As we see in importing the finite amplitude requirement into the concrete model, we are led to an approximate solution to the resulting problem. And in importing more realistic dimensions for the bob and cord, we employ rigid body mechanics with its accompanying idealisations (e.g. a perfect spherical shape for the bob). Finally, in attempting to account for more realistic features of the oscillating pendulum (e.g. the dynamic stretching) we model the result by means of an analogy to another familiar model_I that also carries its own idealisations, e.g. the spring-pendulum model at the near stiff limit. Because the correction factors are numerically small, the remaining idealisations and other physical assumptions (that by no means are exhausted by the examples mentioned) in the concrete model may seem nebulous. But their presence is perspicuous in the claim that model_C is a model that represents a real system. No matter how far we go in bringing the theory close to the phenomena, it seems that the inferential application of claims of the above kind are always subject to the fulfilment of pertinent provisos.

All this reasoning, which is unquestionably not exhausted by the examples given, leads to the conclusion that the family of ideal models may in many instances play a heuristic role in the construction of a concrete model. Nevertheless, no matter how rich the theoretical structure is, the construction itself relies on the ontological commitments of the theory and the auxiliaries used, and on a wealth of physical reasoning that extends beyond the confines specific to the theory and not just on structural features. The positive part to the argument will be pursued further in the next Chapter: that the elements of the processes involved in constructing concrete models, and not some abstract structure that levitates on top of these processes, can teach us about the function of representation models.

4.4 The Restricted Three-Body Problem

The n -body problem in Celestial Mechanics can be formulated (in the Newtonian formalism) as follows: Assume that a system of n bodies consists of point-masses m_i at \mathbf{r}_i , where $i=1,2,\dots,n$, and the \mathbf{r}_i are expressed with respect to an inertial frame of reference. Letting $r_{ij} = |\mathbf{r}_j - \mathbf{r}_i|$, where $r_{ij}=r_{ji}$, then the equation of motion of m_i is

$$m_i \ddot{\mathbf{r}}_i = k^2 m_i \sum_{j=1}^n m_j \frac{\mathbf{r}_i - \mathbf{r}_j}{r_{ij}^3} \quad (4.17)$$

Where k^2 is the Newtonian gravitation constant, and the summation excludes the $j=i$ case. For a complete solution of the n -body problem, $6n$ constants of integration are needed and only ten are known.⁷⁹

For the 2-body problem ($n=2$), equation (4.17) gives rise to the following two equations of motion:

$$m_1 \ddot{\mathbf{r}}_1 = k^2 m_1 m_2 \frac{\mathbf{r}_1 - \mathbf{r}_2}{r^3} \quad (4.18)$$

$$m_2 \ddot{\mathbf{r}}_2 = k^2 m_1 m_2 \frac{\mathbf{r}_2 - \mathbf{r}_1}{r^3} \quad (4.19)$$

These are equivalent to six second-order differential equations that require twelve arbitrary constants of integration for a complete solution. All constants can be found and the problem can be solved analytically.⁸⁰

The 3-body problem ($n=3$) consists of three equations of the following form:

$$m_1 \ddot{\mathbf{r}}_1 = k^2 m_1 m_2 \frac{\mathbf{r}_2 - \mathbf{r}_1}{r_{12}^3} + k^2 m_1 m_3 \frac{\mathbf{r}_3 - \mathbf{r}_1}{r_{13}^3} \quad (4.20)$$

These give rise to nine second-order differential equations. These equations describe the motion of the three point-masses subject only to their mutual gravitational

⁷⁹ See Goldstein 1980 and Danby 1988.

⁸⁰ A detailed solution to this problem can be found in Goldstein 1980, and Danby 1988.

attractions.⁸¹ The problem requires eighteen arbitrary constants of integration for a complete solution. However, only twelve integrals can be found.⁸² Although many particular solutions to it have been found, the general problem remains -and possibly is- insoluble, despite the efforts, for over a period of roughly two hundred years, of a most distinguished list of mathematicians and physicists including Lagrange, Laplace, Jacobi and most notably Poincaré. What is of interest in our discussion is that although perturbation solutions⁸³ can give adequate results, within acceptable experimental error, physicists insist in studying particular solutions to the problem. One of the claims I want to urge is that in cases where exact general solutions to models do not exist and concrete models (in the sense discussed in the previous section, of filling in for the external force function and relaxing idealising assumptions) cannot be constructed, it is from the study of particular solutions that we can gain insight into the physical properties of the motion, and not from just finding an approximate solution to the general problem. In search of such particular solutions, we impose further restrictions to the original model thus narrowing down its domain of application.⁸⁴

⁸¹ In the Hamiltonian formalism the equations of motion are $\frac{dq_{ij}}{dt} = \frac{\partial H}{\partial p_{ij}}$ and $\frac{dp_{ij}}{dt} = -\frac{\partial H}{\partial q_{ij}}$, which give rise to eighteen first-order differential equations, with potential energy V and Hamilton function

$$H; \text{ where, } V = -\frac{m_2 m_3}{r_{23}} - \frac{m_3 m_1}{r_{31}} - \frac{m_1 m_2}{r_{12}} \text{ and } H = \sum_{i,j=1}^3 \frac{p_{ij}^2}{2m_i} + V.$$

⁸² The six integrals of the motion of the centre of mass, the three integrals of angular momentum, and the energy integral, together with the elimination of time and the elimination of what is known as the ascending node.

⁸³ Perturbation calculations can always be used to determine numerically the effects introduced by a third body or even many bodies. Indeed perturbation theory is an essential part of Celestial Mechanics.

⁸⁴ Particular solutions are those that, either the geometric configuration rotates about the centre of mass, or expansions (or contractions) take place in which the mutual distances between the three bodies remain in constant ratios to each other. More generally, they are solutions in which the geometric configuration of the three bodies remains invariant with respect to time. For instance, if the initial conditions are such that the velocity vectors of the three bodies all lie in the plane defined by the bodies, then the motion will always be in that plane.

The *restricted* version of three gravitating bodies treats the case in which the mass of one of the bodies is infinitesimally small (this body is historically known as the planetoid). The two bodies of finite mass move in circular orbits about their common centre of mass under the influence of their mutual gravitational attraction (forming a 2-body system in which the motion is known), and the planetoid moves in their field. Thus, the formulation of the problem introduces a further idealisation to the original problem: the infinitesimal mass does not influence the motion of the other two bodies. In addition, care must be taken to note the restrictions imposed on the system: the two finite-mass bodies move in circular orbits, and the axis joining them also rotates about the same point. Thus, the restricted 3-body problem can be looked upon from another perspective, as an isolated 2-body system into which we introduce one influencing factor, the planetoid. The question asked however is not how the 2-body system is perturbed (since it is assumed not to be), but how a 2-body force field influences the motion of the small body. Looked upon from this perspective the ‘restricted model’ is not a more idealised version of the 3-body model, but a more concrete version of the 2-body model. Although the additional feature of the infinitesimal mass is an idealisation if taken on its own, the actual models to be contrasted are the isolated two-body system with the restricted three-body problem. This I believe is the reason why what physicists call particular solutions are just different techniques of studying some of the physical properties of the system, i.e. a more ‘concrete’ version of a soluble model (the 2-body system) is used to learn about -some aspects of- an insoluble model (the 3-body system).

To set up the differential equations of motion for the restricted version of the problem we may, choose the unit of distance so that the constant distance between the two finite masses is equal to unity, choose the unit of time so that $k^2=1$, and choose the unit of mass so that the sum of the two masses is also unity, where $m_1=1-\mu$ and $m_2=\mu$. Now, if we let the origin of the coordinate system be at the centre of mass and choose axes rotating with the masses such that they both lie along the x-axis then we can derive from the equations of motion of the planetoid and a defined function, known as the modified potential, an equation for the velocity of the planetoid:

$$v^2 = \dot{x}^2 + \dot{y}^2 + \frac{2(1-\mu)}{r_1} + \frac{\mu}{r_2} - C \quad (4.21)$$

C is a constant, and the coordinates of the planetoid with respect to the rotating axes are $\mathbf{r}=(x, y, z)$. Equation (4.21) is known as *Jacobi's integral* of the restricted 3-body problem. For a complete solution of the problem, five more integrals are needed which are not known. Nevertheless, many properties of the motion can be found from a study of Jacobi's integral. Some examples are, Tisserand's criterion for the identification of comets, the surfaces of zero relative velocity, the positions of equilibrium, and the stability points of equilibrium. Let us take a close look at the first of these examples.

If we let the infinitesimal mass have position vector $\mathbf{r}'=(x', y', z')$ with respect to non-rotating axes, with the same origin as before, and let $\hat{\mathbf{z}}$ be the axis of rotation, then

$$\frac{d\mathbf{r}}{dt} = \frac{d\mathbf{r}'}{dt} - \hat{\mathbf{z}} \times \boldsymbol{\rho} \quad (4.22)$$

where $\boldsymbol{\rho}$ is the position vector with components $(x', y', 0)$ or $(x, y, 0)$. Then it can easily be shown that Jacobi's integral takes the following form:

$$\dot{\mathbf{r}}'^2 - 2\hat{\mathbf{z}} \cdot (\mathbf{r}' \times \dot{\mathbf{r}}') = \frac{2(1-\mu)}{r_1} + \frac{2\mu}{r_2} - C \quad (4.23)$$

In this form, the equation can be useful because, if we identify m_1 with the Sun, and m_2 with Jupiter and the planetoid with a periodic comet, then we can identify some of the observables in equation (4.23) with some of the properties of the Sun-Jupiter-comet system. If by observation we can find the position and velocity of the comet at any time, then we can calculate the elements from: $\dot{\mathbf{r}}'^2 = 2/r - 1/a$, and $\hat{\mathbf{z}} \cdot (\mathbf{r}' \times \dot{\mathbf{r}}') = \sqrt{a(1-e^2)} \cos i$, where a is the mean distance from the Sun. Substituting these expressions into equation (4.23), setting r approximately equal to r_1 , and supposing that the comet is observed when far from Jupiter (i.e. *before* a close approach to Jupiter) so that r_1 and r_2 are large and nearly equal, then the result is:

$$\frac{1}{a} + 2\sqrt{a(1-e^2)} \cos i = C \quad (4.24)$$

If we attempt to observe the comet (for a second time) *after* a close approach to Jupiter, since Jupiter exerts perturbations to the motion of the comet, the elements of the comet will have changed. It is possible that they will have changed considerably so that identification of the comet is difficult, i.e. we cannot be sure whether it is the

old comet or a new one. But C is a constant throughout, so if we let a_1, e_1, i_1 refer to the old orbit and a_2, e_2, i_2 to the new orbit, we should (approximately) have:

$$\frac{1}{a_1} + 2\sqrt{a_1(1-e_1^2)} \cos i_1 = \frac{1}{a_2} + 2\sqrt{a_2(1-e_2^2)} \cos i_2 \quad (4.25)$$

This is commonly known as Tisserand's criterion of identification of comets. A number of elements are tacit in this example, there is for instance the blending of the theoretical equation with experimental results. But for this to take place, representation must be partly a matter of denotation, i.e. the elements of our equations must be understood as denoting elements of the physical system, and partly a matter of empirical investigation. Accordingly, Tisserand's criterion is based on an implicit understanding of 'theory' as what the theory is about. This is part of the reason why all the mathematical idealisations and approximations imposed on the system are validated. And it is also part of the reason why, despite the fact that we cannot have a complete solution to the model, we can still use the equation that describes the motion of the planetoid to learn something about the properties of the motion.

But there is one other dimension to this example. The restricted 3-body problem is not just mathematical entertainment for physics students: it demonstrates some of the ways in which we can explore the physical properties of a system. In cases when the most we can do in solving a theoretical model (3-body problem) is a perturbation expansion (which may lead us to accept the adequacy of the model by an approximate match of the model predictions to the experimental measurement), we are left with the task of exploring these properties. This is the reason why the restricted 3-body problem received so much attention. It is a way to reveal some of these properties that are not simply deductive consequences of the model equations. It seems that even in cases where a model is chosen directly from the structure to represent a real system and nothing much can be done to improve the representation (as we were able to do in the pendulum example), we can still break down this model into more specific cases and learn from the latter. We do it because we are in search of the subject matter of the theory, which is its physical content (e.g. the physical mechanisms or properties picked out by its equations or the relata that the elements of its equations denote) and not just the structure (e.g. the relations) displayed by its models.

4.5 Conclusion

I have tried to argue that the relation of model_I to model_C is a complex relation of bringing the theory as close to phenomena as our available tools allow. Implicit in the argument is that these processes are part of our scientific theorising that cannot be overlooked by a simplifying mapping relation between theoretical models and data models. No doubt both theoretical and data models do play their own individual role in the theory/experiment match. But there is an awful lot more that we must keep in mind for an accurate analysis of theory application. We have seen how models in CPM are constructed by physicists for representation purposes and for learning about particular aspects of the physical systems they ostensibly represent. We have seen how these models are loaded with physical parameters that are used because they tell us something about the real system. From this story we begin to see that urging the thesis that these models are somehow related to the corresponding ideal models in the state-space is *a priori* thinking which is validated only by an ideal conception of what a theory is. The physical features of representation models cannot be captured by this kind of understanding of theorising. Van Fraassen's idea that to every concrete model used to represent phenomena there corresponds a model in the state-space, to which the former is an approximant, is an invalidated *a priori* position.

My quarrel with the SV is not about the vehicles of representation. These vehicles are models (although I would be cautious in claiming that models are the only vehicles of representation). The questions I raise focus on what the representation models are, and in particular whether they can be understood by studying the theory on its own. Even if the proposal of the SV proves to be correct about the analysis of the structure of scientific theories, and theories are convincingly shown to be after all families of models united by a common structure, I would still find it difficult to follow their suggestion and understand the relation between theory and experiment as a simple mapping. The representation relation is a complicated matter and it needs an intermediary: the *concrete model*. There is no 'law of theorising' which says that concrete models should be unqualified derivatives of the theory. In fact, there is no reason why they should not include elements that may even conflict (or contradict)

with elements of the theory, if the goal is to understand the world and not just to explore the logical consequences of a particular theory.

We can also see that van Fraassen's understanding of theoretical representation, as a structural relation between a theoretical model and a data model, is unwarranted. The theoretical representation of phenomena by means of models is done by the gradual construction of concrete models. The *terminus a quo* of the latter, may be (in the majority of cases in CPM) theoretical models, but the *terminus ad quem* is a theory-independent construction. The features added to the concrete models in order to make them as accurate representations as possible, show that the questions the scientist is confronted with are about what causal factors and indeed what physical properties are sanctioned to turn a model into an accurate representation of phenomena. Van Fraassen's question of *how the theoretical and data models* represent is, in my view, off the mark. The former represents nothing (in the real world) without the model_c intermediary. The latter does not quite seem to be what the proponents of the SV envisioned. Moreover, a sharp distinction between a theoretical model and a data model seems to exist only in the tidy mind of the 'reconstructing' philosopher.

In the light of the examples and objections presented here, we can make a more definite claim: that -if there is any value to the Semanticist story- the state-space can only be understood (following Suppe) as an abstract and idealised replica of phenomena. However, we must add significant qualifications to this thesis: (1) that the state-space is a structure that unites only the models of the theory, (2) that the laws of the theory can be used to define a finite class of exemplars upon which other models can be modelled, and finally (3) that the representation relation is mediated by the concrete models.⁸⁵ The first and second of these qualifications imply that the scope of the structure is just a handful of abstract and idealised models. The second and third qualifications imply that we use these models as a basis for the construction of representations of the world. The third qualification implies that representations of

⁸⁵ Whether the theory is identified with the state-space (i.e. a structure) or whether mathematical structure is just one of the modes of the theory's equations, is a matter beyond the present discussion. Note, however, that my argument is not conditional on any one of the two assumptions.

the world require the intermediary model_C, into which we may need to utilise the entire conglomeration (or relevant parts) of our background knowledge before we can extend the domain of theorising to the world.

These qualifications obviously diminish the importance of the uniting state-space and shift the emphasis to actual scientific modelling and on how the models represent phenomena. Concrete models are not united by a common structure, albeit only in a loose way, i.e. the exemplars that give rise to them may be such a family. If this is the most defensible thesis for the SV, as I contend, then there still remains an open question. If the theoretical models are not representations, but are abstract and idealised replicas of physical systems, then it is not the replicating relation that needs to be investigated, as Suppe suggests, but the conceptual processes of abstraction and idealisation. To do this we must investigate the abstractive and idealising conditions that are used in our theories, but more importantly, we must investigate how concrete models are constructed. The latter inquiry can assist us in the task of understanding the notions of abstraction and idealisation in science, but also in understanding how the converse process of concretisation functions as a conceptual mechanism in constructing representations of the world.

5 Representation Models of the Nuclear Structure

5.1 Introduction

The birth of early Nuclear physics dates back to the discovery of radioactivity in 1896. During the ‘primitive’ period of development (1896-1932), physicists were primarily concerned with discovering experimental facts about the nucleus. The Rutherford scattering experiments (1911), for instance, showed that an atom consists of a tiny, massive, positively charged nucleus surrounded by a number of light negatively charged electrons. Isotopes were also discovered during this period, the field of mass spectroscopy was developed and a few nuclear reactions were induced in the laboratory. Early attempts to understand the details of nuclear structure by applying the theoretical developments of the period -Quantum Mechanics- were unsuccessful. The reason was that the known experimental facts gave rise, at the time, to the proton-electron picture of the nucleus. These attempts were abandoned even before a new hypothesis was proposed because, among other reasons, early models based on this picture implied that the nucleus should obey Fermi or Bose statistics, depending on whether the number of charges Z in the nucleus was odd or even. It was, however, invariably found that it is the number of proton (nucleon) masses A in the nucleus that being odd or even implies Fermi or Bose statistics, respectively.

With Chadwick’s discovery of the neutron in 1932 (an uncharged particle with approximately the same mass as the proton), Heisenberg and Ivanenko independently

proposed the proton-neutron picture of the nucleus, based on the assumption that a nucleus consists of Z protons that account for the charge and $N=A-Z$ neutrons (where A is the total number of nucleons). Since its proposal, all considerations on nuclear structure have been based on the hypothesis of the proton-neutron picture.

During the time between 1932 and the development of quantum electrodynamics and quantum chromodynamics, several models of the nuclear structure have been developed, all of which are attempts to account for the quantum mechanical features of the nucleus and to explain experimental results. In the early stages of this period, we encounter a pluralistic development of models which explain different properties of the nucleus and which account for different known facts (with occasional overlaps). There exist conflicting models in this list, which nevertheless, since at the time they were used to understand different properties of the nucleus, complemented each other in the attempts to explore the nuclear structure. That they complement each other becomes clear by the fact that they led to the Unified model of nuclear structure, which combined all previous knowledge on the subject. In this Chapter, I shall focus on the development of nuclear models in this period leading to the construction of the Unified model, with emphasis on the different ways by which quantum theory is applied to the nuclear domain and on how the different models relate to each other. Despite the development of quantum electrodynamics and quantum chromodynamics, no ‘unifying theory’ of the nucleus has yet been developed. Because quantum chromodynamics is useful in characterising the strong nuclear force, it may seem promising for providing us with a model of the nucleus. However, it does not yet enable us to construct nuclear models but more importantly, because its scope is confined to high-energy physics it seems to lack the capacity for application to low-energy phenomena, which is also where the demand for nuclear model applications lies. Hence, the models to be discussed are still very valuable not just in assisting us in the classification and explanation of experimental results, but more generally in learning about the properties of the nucleus. In this Chapter I will examine the following models of the nuclear structure: (a) the liquid drop model, (b) the Fermi gas model, (c) the nuclear single particle shell model and some extensions to it. While in the next Chapter I will examine (d) the unified nuclear model that brought together the features of the single particle shell model and of the collective

model. I shall not examine any models of nuclear scattering reactions, e.g. the compound model or the optical model, although I believe that the arguments given here also hold for this set of models.

On the basis of two conflicting hypotheses about the nucleus, we can divide models of nuclear structure and literally all nuclear models into two rather broad categories. The hypothesis that underlies the models in the first category assumes the nucleus as a collection of closely coupled particles; these models are sometimes referred to as the *strong interaction models*. In this set of models the relative motions between the nucleons is entirely ignored and only collective modes of nuclear motion are accounted for. For some purposes, such an idealisation is acceptable because of the large-strength and short-ranged character of nuclear forces. The second hypothesis that underlies the models in the other category assumes that the nucleons move in an average nuclear field in rather independent ways. These ways differ from model to model (within this category) depending on other auxiliary hypotheses constraining the character of the motion. The models in the latter category, in which collective modes of nuclear motion are ignored, are sometimes referred to as the *independent particle models*. Of course, to imply a sharp distinction between the two categories, that physicists often do, is a simplification or an idealisation because almost all of these models contain traces of the characteristics of models that belong to the other category.

The fact that two conflicting hypotheses mark the foundations for the construction of nuclear models may seem *prima facie* as an indicator of an unfortunate truth about the sub-discipline of nuclear physics. Namely, that we lack a comprehensive knowledge of the character of nuclear forces and subsequently that we are not yet able to develop an acceptable ‘unifying theory’ of nuclear structure. But notice that in general even if we did have such knowledge, the task of solving the resulting nuclear many-body problem would still be impossible. Consider the internal energy E of a nucleus of A nucleons, given as an eigenvalue of the Schrödinger equation $\left(i\hbar \frac{\partial \psi}{\partial t} = -H\psi\right)$, where H is the Hamiltonian of the nucleus, the eigenfunction $\psi(r_1, \dots, r_A)$ is the wavefunction, and r_i denotes the position of the i^{th} nucleon:

$$H\psi(r_1, \dots, r_A) = E\psi(r_1, \dots, r_A) \quad (5.1)$$

If we define the *binding energy* B of the nucleus as the minimum energy required to completely separate its component nucleons, and because the potential energy value when the nucleons are separated beyond the range of their mutual interactions is zero, it then follows that $E = -B$.⁸⁶ If we were to apply the Schrödinger equation to compute the eigenvalues for E , we would express the Hamiltonian as a sum of the kinetic energy operator for nucleonic motion and the potential energy operator for interaction between the nucleons, $H = T + V$, where for nucleon mass m :

$$T = \left(-\hbar^2/2m\right) \sum_{i=1}^A \nabla_i^2, \text{ and, } V = \sum_{i>j}^A \sum_{j=1}^A V_{ij} \quad (5.2)$$

The V_{ij} correspond to the interaction potential between nucleons i and j . It is known, of course, that expressing the potential energy as a sum of pair-wise terms is an idealisation. Indeed, influences on the pair-wise interactions are exerted by the presence of other nucleons. This fact, which would strengthen even further the present argument, is presently ignored only to avoid redundant complexities. The nucleus can exist in different bound states, characterised by different wavefunctions and different values of E , as well as of other observable quantities. The different eigensolutions to equation (5.1) correspond to the different states of the nucleus. Consequently, if we could solve the Schrödinger equation, the eigenvalues E would give us the energy of the different states and from the corresponding eigensolutions ψ we would be able to extract all possible information concerning all other conceivable properties of these states. For example, the eigensolutions would provide information about the magnetic dipole moments and the electric quadrupole moments of the nuclei. All this is what quantum theory instructs us; yet solving the Schrödinger equation for the nucleus poses enormous problems. Firstly, the nature of the pair-wise nucleon-nucleon interaction that has to be inserted into the equation is not completely known, nor yet the influence on it from the presence of other nucleons. Secondly, even if this interaction is specified we encounter insurmountable calculational difficulties for the cases of more than two nucleons ($A > 2$), that lead to resorting to variational techniques for solving the nuclear many-body problem.

⁸⁶ Note that the total energy of a stable nucleus is less than the sum of the energy of its constituent nucleons and the difference is the binding energy.

These difficulties are indicative of the fact that none of our available quantum mechanical theoretical models (e.g. the harmonic oscillator, the hydrogen atom, etc.) fit the world of nuclear physics. In order to apply quantum mechanics to the nuclear domain we need to construct the Hamiltonian operator for the nucleus. This work is devoted to how Hamiltonian operators are constructed in the nuclear domain. By exploring the different ways Hamiltonians are constructed, we can learn how quantum theory is applied in the nuclear domain and this, I hold, can subsequently teach us about the relation between quantum theory and nuclear models. The SV teaches us that models are the result of mathematical definition (given the laws of the theory). I find this claim an obscuring simplification, if it is used descriptively (i.e. to describe actual model construction), and I find the SV in need of being supplemented with a ‘theory’ of model construction.⁸⁷ In actual practices, such as the application of quantum theory in nuclear physics, what the SV calls a ‘definition’ involves a complexity of conceptual activities. The construction of nuclear models, in particular, involves (for example) positing novel hypotheses about the structure and properties of the nucleus and synthesising these with already established hypotheses. The SV also teaches us that the unity of theory and models is manifested by the subsumption of all models under the unifying theory structure. This is also a claim that seems untenable for the case of nuclear models. If there is a form of unity, it seems to me, we must search for it not in structural analyses but in more complex relations that may hold between theory and models.

In earlier Chapters, I charged the SV that it presupposes a sharp distinction between models of theory and models of data, and that it assumes that when we define a theory-structure we immediately lay down an indefinite number of models that are available for modelling the theory’s domain. These assumptions imply that theory application is achieved by choosing a model of the theory and contrasting it to the

⁸⁷ Admittedly the da Costa/French line, which is not discussed in this work, is an attempt to do this by supplementing the SV with an account of heuristics. Whether the partial structures account is successful or not in addressing the problems raised here is subject to further work beyond the scope of this dissertation.

appropriate data model. If the SV rested merely upon these assumptions then the task of countering it would not pose significant difficulties. There are numerous applications of quantum theory, among them nuclear physics, where models of the theory are not used to represent physical systems. But the SV rests on an additional assumption. It also assumes that the actual scientific models used in theoretical representation approximate a model of the theory, or that they are the pragmatic counterparts of corresponding models of the theory. A consequence of this assumption is that the methods and processes of construction of actual representation models can be ignored, for they bear no effect on how these models function in scientific inquiry. Hence the SV renders these processes philosophically uninteresting and obscures that representation models often are constructed in ways that are partially theory independent and often function in theory independent ways.

In Chapter 4, I challenged the view that representation models can be identified with approximations of theoretical models in any straightforward way because, representation models depart in significant ways from the initial theoretical models that gave rise to them. My argument, however, could not stretch far enough since the scope of CPM is such that for most cases a theoretical model is available for employment in the construction of a representation model. In contrast, the sub-discipline of nuclear physics can be best understood as an area where we lack any useful theoretical models and we therefore attempt to construct representation models with a minimal reliance on theory. We lack, that is, a model analogous to the classical linear harmonic oscillator, which we can use to construct a representation model for the simple pendulum. Nuclear physics is therefore an area where representation models are related to the theory in ways that the SV offers very little help in understanding. Moreover, it is an area in which we can see clearly the incapacity of quantum theory to represent without the intermediary concrete representation models of the nucleus. Underlying my arguments throughout this Chapter and the next is, therefore, a much stronger claim: that representation models cannot be identified with any sort of approximation of theoretical models, unless our claim is loaded with unjustified *a priori* suppositions.

To argue for such a claim, I depart from the SV by holding on to the premise that the processes of construction of representation models are important and operative to how the models function. I shall try to demonstrate that models of the nuclear structure are not constructed by an outright reliance on quantum theory, i.e. that the construction of representation models is not achieved by having a model of the theory as a starting point. Because of the difficulties involved in applying quantum theory (directly) to the nuclear domain, representation models of the nuclear structure cannot be constructed by having as basis an antecedently available class of models. In this domain, the processes of construction are far more complex. I shall describe two kinds of such processes. In the first, we set-up a classical model and then convert its parameters to their quantum mechanical analogues to obtain the Hamiltonian operator, thus importing the model into quantum theory. In the second, the ideal models of quantum mechanics are in constant interplay with postulated mechanisms specific to the nuclear domain. There are two elements present in the latter kind of construction. On the one hand, the postulated mechanisms are frequently *ad hoc*, and on the other hand they instigate the construction of new ideal models, thus enhancing the scope of the theory.

Because none of our available quantum mechanical theoretical models fit the world of nuclear physics, I argue that we cannot rest upon the conjecture that the models constructed in this domain to account for nuclear structure, as well as those that account for nuclear scattering reactions, somehow approximate ideal models of quantum theory. This cannot be justified because it would require that we have some knowledge of the physics in the nuclear domain. But the goal of our inquiry into this domain is to learn about the physics of the nucleus and indeed the nuclear models are one of the means by which this goal is facilitated and partially achieved. The domain of nuclear physics is such that we must put together every bit of our knowledge in order to explore it. One of the major tasks in this exploration is to construct concrete models that enable us to improve our insight into the physics of the nucleus. Contrary to the SV, I dispute the claim that this improvement comes about by subsuming the nuclear models under a unifying theory structure. My claim is that we improve on the representation capacity of nuclear models by relying heavily on the predecessor models and thus improving on the explanatory and predictive success of the models. I

consider the development of nuclear models an example in science, which indicates that the scope of quantum mechanics is not, in any clear sense, some physical domain like the nucleus. Assuming, therefore, that the scope of a theory is the class of its semantic models, it follows that the theory cannot represent the nuclear structure without the intermediary representation models. This immediately leads to questions pertaining to the application of the theory and also to questions of whether the understanding of theory application, suggested by the SV, does justice to the nuclear case. This is from where I derive my first motivation for exploring how quantum theory gets applied in the nuclear domain; or, as I prefer to phrase the aim of my inquiry: how quantum mechanics is used in the construction of representation models of the nuclear structure.

My second motivation, tacit throughout this work, is that to understand what a theory is we must look at how it is applied. Hence, I see the exploration of the nuclear domain not only instructive for our understanding of theory application, but also as a means to shed light on what the nature of scientific theories is. The Semanticists, as well as the Logical Positivists, maintain the opposite, i.e. that we first establish our understanding of what a scientific theory is -by some form of formal reconstruction- and then incorporate into this framework all its applications. Because nuclear physics does not demonstrate a final product, i.e. a complete ‘unifying theory’, that philosophers may reconstruct as they wish, it is easy to discern the various stages of theoretical development in this domain. Subsequently, the various methods and processes employed in the construction of nuclear models (which are also discernible) reveal how quantum theory is applied and its domain of application extended to cover different domains and new phenomena. Contrary to the SV, these processes do not indicate a unity of theory and models by appeal to mathematical structure.

Nuclear physics is a problematic case for the proponent of the SV. There are very few signs of structural unity of the nuclear models. For instance, there is a number of conflicting features in different models. It is an area that the proponents of the SV must fit into their conception of scientific theories. If Quantum Mechanics is merely a class of structures united by a Hilbert space, then surely its application to nuclear

phenomena (via the models to be discussed here) must indicate just that. It is, in other words, a pending job for the proponent of the SV to show how exactly the nuclear models are models of the theory. It will be argued, in the sequel, that nuclear physics does not point to that direction. Various techniques are involved in constructing representation models of the nuclear structure, such as analogies with well-established ideas from disparate scientific branches. To give some examples: (1) in the liquid drop model the nuclear behaviour is assumed analogous to the hydrodynamics of a liquid drop, (2) in the various shell models of the nucleus the nucleons are assumed to occupy shells analogous to the stationary orbits of electrons in an atom, (3) in the optical model the nucleus is treated as a refractive medium. By its reliance on structural mapping of theory to phenomena and by its demand for structural unity of the models, the SV would render all of our nuclear models, in an ultimately trivial and misguided manner, unrealistic. Even those models (such as the unified model), which seem promising in guiding us towards a reasonably accurate representation of nuclear structure.

It is customary among physicists to call the existing nuclear models phenomenological. This terminology is due to a number of reasons: (1) because the models are constructed by the deployment of semi-empirical results, or (2) because the Hamiltonian operators used are established in *ad hoc* ways to explain particular phenomena related to the structure of the nucleus or to scattering experiments, or (3) because the concepts used in their construction are not always directly related to the fundamental concepts of the theory. In other words, physicists consider these models phenomenological because they are not in any straightforward sense deductive consequences of quantum theory. This, of course, does not mean that they are dislocated or dissociated from the theory. In fact, quantum theory is the basis for the construction of each and every one of the nuclear models. The customary labelling of the nuclear models as 'phenomenological', does not lie so much in our lack of understanding how they relate to the theory (although, admittedly, in some cases we do lack this understanding), as in the expectation that representation models should be deductive consequences of the theory. The Logical Positivists as well as the proponents of the SV have built their views of scientific theories on this intuition. In the framework of the SV this intuition translates into the expectation that a

representation model belongs to the family of theoretical models that constitutes the theory, or that it relates in some approximate way to one of these models. I see my work, as a challenge to the proponents of the SV to show and justify that this is so for the models we construct to represent nuclear structure.

The argument in this Chapter is characterised by two dimensions. The negative dimension is the attempt to criticise the SV on several fronts. The positive dimension is the attempt to understand what representation models are, how they relate to theory, how they are constructed and how they function. I will try to explicate the argument's structure by stressing my primary focus in each individual section. The primary intention in Section 5.2 is to stress the claim that a theory gets applied to a certain domain via its semantic models only on rare occasions and only under highly idealised assumptions, or by imposing on the physical system conditions that are known to be fictitious. Furthermore, I describe the two kinds of model construction mentioned above, and emphasise the fact that representation models are constructed using anything we can gather from our background knowledge, and then imported into the theory in ways for which no definite theoretical justification exists. In this section, I also develop the three most important early models of nuclear structure: the liquid drop model, the Fermi gas model, and the single particle shell model. In Section 5.3 the argument focuses on the case where we impose a description on the physical system, to which we can assign a semantic model from the outset. I then try to show that to turn this initial model into a representation device, in particular for unexplored physical domains like the nuclear case, very frequently we must postulate in *ad hoc* ways mechanisms that cannot necessarily be subsumed under a unifying theory structure. Yet, these mechanisms give the representation model its wanted explanatory and predictive power. In order to justify these mechanisms we pursue routes where the theory plays a minimal role, e.g. experiment. I explore in particular the spin-orbit hypothesis that was introduced to give the shell structure hypothesis for the nucleus predictive and explanatory success. The argument continues into Chapter 6 where I argue that the Unified model, which is the most sophisticated model of nuclear structure that has been constructed, synthesises all of our past knowledge about the nucleus, that was gained by the explanatory and predictive successes of earlier models.

Underlying my explication of the construction of the nuclear models to be discussed are several equally important considerations that run parallel and are of primary significance to my main arguments. The first of these is to bring to the surface what we can infer about the predictive and explanatory successes of models for some nuclear phenomena, and in particular to explore why their predictive and explanatory successes render the models independent, at least partially, from the theory that gives rise to them. The second is to stress that there is an implicit reliance, in various forms and ways, of the development of each model on its predecessor models, a point that becomes explicit in Chapter 6. The third is to bring forth some of the different ways by which the theory is deployed in the construction of representation models and thus its domain of application extended beyond the class of its semantic models. The fourth is to emphasise the fact that the SV cannot justify the application of a theory on the basis of a structural relation between a theoretical model and a data model, and that the SV does not do justice to scientific inquiry into the nuclear domain by adopting the view that every representation model is an approximant of a model of the theory. I also try, when it is useful for my argument and when the outcomes of my investigation allow it, to expose other untenable implications of the SV. Finally, I attempt to keep to the historical dimension of the development of nuclear models with reverence, despite the fact that on occasions the order of the presentation violates the historical order of the development. At all times I try to keep clear in the argument my thesis, that each nuclear model is a complex product of the attempt to extend quantum theory to the nuclear domain and of a salient reliance on the predecessor nuclear models.

5.2 Early Models of Nuclear Structure

The early conflicting physical intuitions about the nature of the nucleus (i.e. the strong interaction and the independent particle hypotheses), gave rise to two sets of models that demonstrate two distinct ways by which representation models may be constructed. Each of these processes of model construction is a function of the nature

of the underlying model hypotheses. The nature of the underlying hypotheses imposes particular ways by which the model may be imported into the theory. The strong interaction hypothesis dictates a description of the physical system that leads to the construction of a classical model, which later leads to a kind of ‘theory entry’ that involves assigning quantum properties to the classical parameters of the model. The independent particle hypothesis dictates a description of the physical system, which allows the use of a theoretical model in the construction of the representation model. Both of these processes of construction impose their own heuristic, indicating the different heuristic usage of theory to achieve the goal of constructing a representation model. In sub-section 5.2.1 we look at the first of these processes and in sub-section 5.2.2 we look at the second.

5.2.1 Early Strong Interaction Models: The Liquid Drop Model

One of the first and simplest nuclear models to be proposed was the *liquid drop model*.⁸⁸ The hydrodynamic analogy between nuclear matter and a liquid drop is suggestive of the basic assumption that underlies the model. The mean free path of nucleons must be significantly small compared to the nuclear radius, just as the mean free path of molecules in a liquid drop is small compared to the radius of the drop.⁸⁹ This assumption implies that the nucleus can be regarded as a system of closely coupled particles, where independent motions of the constituent particles are ignored. At the time when this model was proposed, this hypothesis seemed plausible in view

⁸⁸ See Bohr 1936, Bohr and Kalckar 1937, Bohr and Wheeler 1939. To be historically accurate another strong interaction model of nuclear structure known as the *alpha-particle model* preceded the liquid drop. In this model the nuclear constitution is assumed to be alpha particles, which are thought to be basic stable sub-units. The alpha-particle model was an attempt to use an established model of quantum theory to describe collective modes of nuclear motion. Motivated by the exceptional stability of alpha particles this model was partially developed and used to explain several structural as well as scattering phenomena. As the proton-neutron hypothesis prevailed and became widely accepted, the alpha-particle model was given up.

⁸⁹ The analogy of nuclear matter with a liquid drop is also suggested by the fact that density and average binding energy per particle are approximately constant for all except the lightest nuclei. This is known as the saturation property of nuclear matter.

of the increasing knowledge of the large-strength and short-range nuclear forces. Since then the successes of later independent particle models have led to the conclusion that accounting for independent nucleonic motion provides us with considerable insight into many other aspects of nuclear structure.⁹⁰ Nevertheless, the liquid drop model, which exemplifies strong interaction models, offers considerably successful quantitative results for nuclear characteristics such as binding energies, nuclear radii and collective oscillations and rotations, and accounts well for such physical processes as nuclear fission. Let us look into some of these in some detail.

Before the proposal of an adequate nuclear model, with the development of mass-spectroscopy it was found that the nuclear mass is related to the masses of its constituent particles and the nuclear binding energy: $M_{nuc} = ZM_p + NM_n - c^{-2}B$, a result which shows that nuclear binding energies are sufficiently large to affect nuclear mass. Another surprising result about nuclear binding energies is their approximate constancy for different nuclei (except the lightest). Along with other experimental results, these led in 1935 to von Weizsäcker's semi-empirical mass formula (also arrived at, independently, by Bethe in 1936):

$$B = C_{vol}A - C_{surf}A^{2/3} - C_{coul}Z^2A^{-1/3} - C_{sym}(A - 2Z)^2A^{-1} - C_{pair}A^{1/4}\delta \quad (5.3)$$

In the following discussion the terms in the Weizsäcker formula are treated in the appeared order. The first three terms are just of the form suggested by the classical analogy with the charged liquid drop. If we consider an infinitely extendible liquid (of constant density) then the energy would be proportional to the number of particles. In the nuclear analogy this volume energy is the average energy due to saturated bonds between the nucleons, which contributes to B . But since the nucleus is finite, the nucleons near the surface should interact with fewer nucleons (i.e. there should be unsaturated bonds). Thus B should decrease by an amount proportional to the surface area, i.e. to $A^{2/3}$. Furthermore, the binding energy reduces more on account of the Coulomb repulsion between any two protons. This is inversely proportional to the distance between two protons, which turns out to be inversely proportional to $A^{1/3}$.

⁹⁰ In fact, we shall see later that the types of motion the nucleus exhibits are a mixture of collective and single-particle modes. Thus, both kinds of early models involve different kinds of idealisation.

At this point the classical analogy ceases to help, but the following considerations suggest the addition of the last two terms. The tendency of nuclei to have equal numbers of protons and neutrons gives rise to the symmetry term which for $Z=N$ diminishes. Finally, a pairing term must be added in order to reproduce the special stability of even-even (for Z and N respectively) nuclei and the almost complete absence of stable odd-odd nuclei. Thus in the Weizsäcker formula, $\delta=+1$ for odd-odd nuclei, $\delta=0$ for odd A nuclei, and $\delta=-1$ for even-even nuclei.

Although the liquid drop model is a valuable guide to constructing the Weizsäcker formula,⁹¹ it is patent that more detailed models are required to relate the magnitudes of the various terms to the basic interactions between nucleons. Nevertheless, the success of the formula in yielding relatively accurate values in most cases and in reproducing all important trends, except for the lightest nuclei, can therefore be regarded as an indicator of the relative success of the model. One such success of the model is in providing an explanation for the phenomenon of nuclear fission of heavy elements, the discovery of which came in 1939 and enhanced research into strong interaction models.⁹² Nuclear matter is assumed to be incompressible, just as a liquid almost is, but deformation is possible. If a spherical nucleus is deformed into an elongated shape the following things would happen. First the Coulomb repulsion is diminished because the average distance between protons increases. Second the surface energy increases because the surface area increases. These two changes, that have opposing effects on the magnitude of the binding energy, mean that heavy nuclei will demonstrate instability against deformation. This is so because the Coulomb energy increases with Z^2 , whereas the surface energy increases with $A^{2/3}$, hence for large Z the Coulomb energy will take over. For light nuclei, on the other

⁹¹ For the sake of historical accuracy, one should say that the model is valuable for *reconstructing* the formula, i.e. explicating the binding energy in terms of the constituent parts of the formula, as the Weizsäcker formula historically preceded the exact formulation of the model. It is my personal opinion that it also was a precursor and stimulus to the construction of the model.

⁹² As a historical note, four years earlier in 1935 a German chemist, Ida Noddack, proposed a fission explanation of Fermi's 1934 experiments, immediately following their announcement. Ironically, it was fortunate for humanity that her ideas were encountered with scientific 'conservatism' and dismissed at the time.

hand, the surface tension is more significant hence the spherical shape is the stable configuration. A deformation of a large nucleus, whether spontaneous or initiated by the capture of a particle, may therefore lead to a large deformation and subsequently to a split-up into two or more parts of comparable mass. The liquid drop model also provides, to a first approximation, good quantitative results for fission. However, some important properties of the nucleus are not adequately accounted by the model, for example the special stability of the ‘magic-number’ nuclei, and fluctuations of the pairing energies.⁹³ But the primary purpose for discussing the liquid drop model is not to argue about its infallible predictive and elaborate explanatory power. In addition to stressing that the liquid drop gave birth to strong interaction nuclear models, it is to show how we can use a set of classical hypotheses to set-up a classical Hamilton function as a starting point, and then incorporate quantum mechanical features into the model. To demonstrate this we must look at a quantitative application of the model where the nucleus is treated quantum mechanically.⁹⁴

According to the liquid drop model, because any energy acquired by a nucleon is quickly shared, nuclear excitations involve collective displacements of many nucleons. Thus in this model, the motions of individual nucleons are completely ignored and the nuclear wavefunction is entirely described in terms of the position of the nuclear surface. If we assume at the outset that the nucleus in its stable state has spherical shape with surface radius R_0 , then for small deviations from sphericity (where the surface undergoes deformation oscillations at constant density, in which the surface tension of the nucleus acts as a restoring force), the equation for the surface can be written as follows:

⁹³ For certain numbers of protons or neutrons the nuclei demonstrate distinctive stability. These numbers are known as the ‘magic-numbers’: 2, 8, 20, 28, 40, 50, 82, 126. The same phenomenon was known to be demonstrated by atoms. Pairing energy fluctuations can be understood as an increase or decrease in the binding energy due to the tendency of nucleons of the same kind to pair-off in the nucleus. The binding energy increases for even-even nuclei and decreases otherwise due to the odd nucleon. It turns out that when two nucleons with different total angular momentum eigenstates (where the difference lies in the sub-states) pair-off, their pairing state has zero total angular momentum.

⁹⁴ Detailed expositions of the liquid drop model can be found, among many other sources, in Moszkowski 1957, von Buttler 1963, and Segrè 1977.

$$R(\theta, \varphi) = R_0 \left[1 + \sum_{\lambda} \sum_{\mu} \alpha_{\lambda\mu} Y_{\lambda\mu}^*(\theta, \varphi) \right] \quad (5.4)$$

In this defining equation, the $\alpha_{\lambda\mu}$ are deformation parameters (amplitudes of oscillation) whose values determine the nuclear shape, and the $Y_{\lambda\mu}$ are spherical harmonics. In accordance with the liquid drop model, the energy of the nucleus is the sum of the volume energy, surface energy and Coulomb energy. On the assumption of incompressibility, the volume energy is independent of the nuclear shape. The surface energy is least for spherical shape and increases with deviation from sphericity. The Coulomb energy on the other hand decreases with deviation from spherical symmetry. Given these considerations for the energy, for small deviations from sphericity, the nuclear energy according to the model is of the form:

$$E(\mathbf{a}) = E(0) + \frac{1}{2} \sum_{\lambda} C_{\lambda} \sum_{\mu} |\alpha_{\lambda\mu}|^2 \quad (5.5)$$

In this equation, \mathbf{a} denotes the set of deformation variables, $E(0)$ the energy for spherically symmetric shape, and C_{λ} are nuclear-deformation-resistance coefficients. The C_{λ} are classical coefficients that can be computed by elementary reasoning in geometric and electrostatic terms. The deformation parameters $\alpha_{\lambda\mu}$ however, are initially treated as classical time-dependent spherical tensors, that will be given quantum properties by applying -what seem to be the standard rules of- quantum mechanics at the end-point. To consider the effect of variation on the deformation parameters, we can look at surface oscillations.⁹⁵ If the nuclear surface changes slowly in some prescribed way, there will occur a collective flow of nuclear matter in the interior of the nucleus. Assuming irrotational flow, we can approximately (to first order in the $\dot{\alpha}_{\lambda\mu}$) define the velocity field at every point inside the surface as:

$$\mathbf{v} = R_0^2 \sum_{\lambda} \frac{1}{\lambda} \sum_{\mu} \dot{\alpha}_{\lambda\mu} \nabla \left(\left(\frac{r}{R} \right)^{\lambda} Y_{\lambda\mu} \right) \quad (5.6)$$

⁹⁵ There are four important types of nuclear collective motion (i.e. where many nucleons move coherently with well-defined phases), *surface vibrations*, *rotations* of deformed nuclei, the various deformation stages of *nuclear fission*, and collective behaviour in the nuclear interior commonly known as *giant resonance* motion. Since these types will be brought together later in the discussion of the unified model, the focus in the above example is confined to surface vibrations and, for simplicity, combinations with any other type of motion are ignored.

For slow changes in the deformation parameters, the total kinetic energy of the mass flow throughout the nucleus is then of the form:

$$T = \frac{1}{2} \sum_{\lambda} B_{\lambda} \sum_{\mu} |\dot{\alpha}_{\lambda\mu}|^2 \quad (5.7)$$

The quantities B_{λ} are the mass parameters whose calculation depends on the assumption of irrotational flow and on equation (5.5), although the latter dependence is not here shown explicitly. The Hamiltonian for surface oscillations is given by:

$$H = T + E(\mathbf{a}) \quad (5.8)$$

Where, the term $E(\mathbf{a})$ plays the role of the potential energy (calculated as the work done against the surface tension in the deformation, as in equation (5.5)) for the collective motion, and the kinetic energy T is the excess of the actual energy over the value of $E(\mathbf{a})$, which would result if the nucleus were static. Now, for small oscillations equation (5.8) becomes

$$H = E(0) + \sum_{\lambda} \sum_{\mu} \left(\frac{1}{2} B_{\lambda} |\dot{\alpha}_{\lambda\mu}|^2 + \frac{1}{2} C_{\lambda} |\alpha_{\lambda\mu}|^2 \right) \quad (5.9)$$

The oscillations of the system are the same as for a particle in a many-dimensional harmonic oscillator potential. The equation of motion may be quantized -in the usual way- by introducing momenta $\pi_{\lambda\mu}$, canonically conjugate to the $\alpha_{\lambda\mu}$, so that the Hamiltonian operator takes the form:

$$H = E(0) + \sum_{\lambda} \sum_{\mu} \left(\frac{1}{2} \frac{|\pi_{\lambda\mu}|^2}{B_{\lambda}} + \frac{1}{2} C_{\lambda} |\alpha_{\lambda\mu}|^2 \right) \quad (5.10)$$

The energy levels of the complete Hamiltonian are then given by:

$$E = E(0) + \sum_{\lambda} \sum_{\mu} (n_{\lambda\mu} + \frac{1}{2}) \hbar \omega_{\lambda} \quad (5.11)$$

Where, each of the $n_{\lambda\mu}$ may be regarded as the number of oscillator quanta in the mode $\lambda\mu$, and the $\omega_{\lambda} = \sqrt{C_{\lambda}/B_{\lambda}}$ are the classical oscillation frequencies.

In trying to bring forth some of the elements involved in the construction and use of this model, we cannot fail to immediately notice the resulting outcome (i.e. equation (5.10)) and how significantly different it is from the form of equation (5.2). It could be said that this is, obviously, primarily due to the fact that nucleon-nucleon interactions are completely ignored in the liquid drop model. But there is also a methodological dimension that is often suppressed. In our quest to assign a

Hamiltonian operator we begin with certain considerations about the physical system. In the case of the liquid drop model we begin with classical considerations and quantize the equation of motion in the end (classical considerations are also employed for the energy calculation due to nuclear rotations and then by quantization the eigenvalues of the rotary energy are obtained). This is not to say anything about the realistic status of the model, but it is to emphasise the process of ‘theory entry’. We know that the nucleus is better described as a quantum mechanical system. But our knowledge of the nuclear structure is too lacking both in respect and degree, for us to be able to employ a quantum mechanical description from the start. A quantum mechanical description of the collective motion of the nucleus would enable us to assign to the system a quantum mechanical Hamiltonian directly without first resorting to classical assumptions. Why is such a direction not pursued? Why do we instead start from classical hypotheses, set up a classical model, which is not any different from a classical spherical vibrating membrane, and then quantize the quantities of the model?⁹⁶

One of my claims, in a different context, throughout Chapter 4 has been that the SV cannot address questions of such nature. Indeed, such questions load us with the burden of having to account for how the concrete models we use for representational purposes relate to our entire theoretical edifice. The SV obscures the need for such exploration. It instructs us that the theory is the class of its theoretical models, and that they can be applied to all physical domains that belong to the scope of the theory. The application of a theory in a particular domain rests upon a theoretical hypothesis, which asserts that the model used relates in some structural way to the data model of the physical system it purportedly represents. The actual model used for this purpose is, in its turn, some approximate form of a theoretical model. All that characterises the application of a theory, according to this view, is to simply look through the models of the theory and choose the one that best matches the description of the physical system. I do not think that this approach does any justice to the liquid drop model. We cannot consider it as approximating any model of a theory. It is what we

⁹⁶ These questions will be re-addressed in the next sub-section 5.2.2.1, in the context of comparing the process of theory entry of the liquid drop model to that of the Fermi gas model case.

would call a semi-classical model. But in that case which theory would we be searching in, the classical or the quantum theory? The proponent of the SV is left with two choices.

The first is to find reductive rules by which to assign, in a systematic theoretically justified way, quantum mechanical properties to classical variables; or in simpler words, rules by which to map classical functions to quantum mechanical operators. To the best of my knowledge, attempts to formulate such rules have so far failed, and the transition from the classical model to its quantum mechanical ‘counterpart’ remains theoretically unjustified and to a large degree arbitrary. We rest our bold ‘quantization leap’ on the arbitrary assertion that ‘this is a suitable place to go from the particular to the general’, and on the likewise arbitrary assertion that equation (5.10) is valid when equation (5.9) is not. This arbitrariness in our procedure of importing the model into the framework of quantum mechanics is warranted only by the fact that the resulting model is successful in predicting experimental measurements and explaining empirical phenomena. This point is about ‘theory entry’; we are just unable to give a description of the collective modes of motion of the nucleus such as to be able to adopt one of the semantic models of quantum theory. Nevertheless, representation models do not always come in such a theory-regulated manner, and this is one way by which they exhibit theory independence and by which they extend the domain of application of the theory.

The second choice for the proponents of the SV is to appeal to the model’s phenomenological or semi-empirical standing, in which case we would be forced to scrap the model altogether. But in that case, we would also be discarding its explanatory and predictive power, and more importantly we would be overlooking, or assigning no relevance to, its influence on successor models of the nucleus. We would, in other words, be forced to dismiss altogether the explanatory and predictive value of other more sophisticated models, like the unified model. Because, as we shall see in Chapter 6, parts of the Hamiltonian operator of the unified model are constructed along the same lines as that of the liquid drop model. This attitude would lead us to dismiss the entire nuclear model research program. A research program that according to Bethe, one of the most distinguished theoretical nuclear physicists, has

consumed the most intellectual energy than any other that preceded it. But this, to say the least, naïve attitude is unquestionably unjustified in view of the physical knowledge about the nucleus acquired via the research program.⁹⁷

5.2.2 Early Independent Particle Models

5.2.2.1 The Fermi Gas Model

The Fermi gas model was one of the earliest attempts to incorporate quantum mechanical features into the discussion of nuclear structure.⁹⁸ In the independent particle models, it is presupposed that the effective mean free path of nucleons (against collisions) within the nucleus is at least comparable to the nuclear diameter. A popular way of saying this is that the nucleons move approximately independently within the nucleus (i.e. with largely uncorrelated motions or in essentially undisturbed orbits). This assumption is, evidently, the very opposite of the underlying assumption of the liquid drop model. Another accompanying assumption, common to all independent particle models, is that the lowest modes of excitation involve a change in the wavefunctions of only one or a small number of nucleons (unlike strong interaction models where it is assumed that the easiest excitable degrees of freedom involve a large collection of nucleons). The additional presupposition, peculiar to the Fermi gas model, is that the wavefunctions of the individual nucleons are held to be plane waves. This is unquestionably a highly idealised assumption, because it implies that the nucleons move in a nuclear zero-potential field of infinite dimension.

⁹⁷ I also find the SV lacking in another respect: it fails to account for the creativity involved in using all background knowledge to construct the liquid drop model (Classical Mechanics is here understood as background knowledge in modelling the nuclear collective motion), and also that involved in importing the model into the prevalent theory. But this is a side issue, which I do not intend to pursue.

⁹⁸ See Moszkowski 1957. In fact, the Fermi gas model (as well as the single particle shell model, the discussion of which follows) chronologically preceded the liquid drop model. This historical detail, however, is irrelevant to the concerns of my discussion, hence my choice of a reverse order in the presentation of the models.

A qualitative description of the model is more appropriate, as the mathematical formalities involved are straightforward and generally well known (the ‘equivalent’ in the shell model research program will be dealt with in more detail later). Beginning with the assumption that nucleons are plane waves, the energy of the nucleons is assumed at the outset to consist only of a kinetic energy term. Strictly speaking, the plane wave assumption is valid for a hypothetical nucleus of infinite extent. As long as the effects of the nuclear surface are not considered however, it can be used to approximate finite nuclei too. To compute, for example, the energy of the nucleus we write the Hamiltonian operator as the sum of the kinetic energies of the individual nucleons plus the sum of the potential energy interactions between pairs of nucleons, as in equation (5.2). On the plane wave assumption we have an expression for the kinetic energies (non-relativistic case), hence the only significant problem is the calculation of the pair-wise interactions. For this we simply calculate two-particle wavefunctions, which are essentially the product of two one-particle functions, that account for the four different spin combinations and the charge effect on a proton-proton interaction. It is not difficult to arrive at an average computation of the binding energies. If we want to separate the binding energy into the terms of the Weizsäcker formula, additional assumptions must be made as to account for each of the terms. For example, accounting for the surface energy term requires that we impose further constraints on the plane wave assumption by assuming that the nucleons move in a box, which is equivalent to postulating a nuclear infinite square-well potential. Various other (phenomenological) corrections can be made to the model that eventually turn it into a good predictor of low-energy nuclear phenomena, such as, a good approximation of average nuclear binding energies and also other low-energy nuclear properties.

For the construction of the Fermi gas model we obviously employ one of the ‘stock models’ of quantum mechanics, namely the one associated with the kinetic energy operator. ‘Stock models’ is the term used by Cartwright (1999) to refer to what I have earlier in this work called, using a more familiar Kuhnian term, ‘exemplars of the theory’. Building on her earlier work (1983), Cartwright argues that theories give us a finite class of semantic models (she calls them *interpretative* models), its *stock*

models. The idea is similar to the one advocated in Chapter 4, that representation models are not to be found in the womb of the theory. I proposed this idea rather briefly at the end of Chapter 4: the stock models are those supplied by the theory upon which other models can be modelled. Cartwright's concerns are also about the construction of representation models, which she recognises that in most cases is not accomplished by searching through the semantic models of the theory to find one that matches the description of the physical system. She proposes that to understand the processes of these constructions we view the theory in conjunction with its *bridge principles*. Bridge principles, in Cartwright's use, do not have the same function as for Hempel (explained in Chapter 2). For her, they are the means by which the abstract terms of the theory get applied to more concrete terms contained in the semantic models of the theory, that can reasonably be associated with actual physical systems. As I shall avoid elaborating on Cartwright's abstract/concrete distinction in the theoretical concepts of a theory, I shall simply employ an understanding of bridge principles as a means of application of a theory. But this form of application must be understood as one of many forms, a theoretically principled form (as Cartwright calls it) of applying the theory. A bridge principle functions as a licence for employing a stock Hamiltonian of the theory, but this only happens when the description of the physical system is such that it allows it. Cartwright takes a further step and urges us to view the scope of the theory as exactly that which its bridge principles tell us: 'In so far as we are concerned with theories that are warranted by their empirical successes, the bridge principles of the theory will provide us with an explicit characterisation of its scope. The theory applies exactly as far as its interpretative [semantic] models can stretch. Only those situations that are appropriately represented by the interpretative models fall within the scope of the theory.' [Cartwright 1999, p196] The bridge principles of a theory, according to Cartwright, are few in number hence the scope of a theory is highly restricted. This is why representation models are so valuable as intermediaries in theory application -they extend the domain of application of the theory.⁹⁹ But could we consider them as

⁹⁹ Occasionally a new stock model is defined for use in a particular domain, but the expansion of the stock model list is unquestionably very gradual. Most importantly, this expansion takes place because particular *representation models* instigate it. I shall be examining such a case in section 5.3.

extensions of a theory? I think not, but even if we did we should do that with a number of antecedents in mind that would radically alter the meaning of ‘extension’ from the deductive sense implied by the SV, or indeed the logical positivist view. I take from Cartwright that only on rare occasions do the bridge principles of a theory licence the use of a stock model in the representation of a physical system. In the majority of cases physical systems do not have such ‘licensing’ descriptions, and in the cases when they do extravagant idealisations and abstractions are involved. I depart from Cartwright on two grounds: on my understanding that a theory gets applied not just through its stock models but also via a variety of other means (e.g. the liquid drop model) and on my understanding of how representation models mediate between theory and the world. I consider representation models as partially independent from theory only in the sense that they may function on their own as devices for explanation and prediction, partially independent of the success of the theory that gave rise to them. I also consider representation models as the offspring of theories supplemented by additional physical reasoning, a view that I hope to bring to the surface in the sequel. Nonetheless, the idea I wish to explore is that representation models very rarely come into being in the theoretically principled manner the SV instructs us.

At this stage I want to raise the following point as a follow-up to what was mentioned in the previous section about the liquid drop model. Just like the liquid drop model, the Fermi gas model marks the basis of successor nuclear models. The Fermi gas model, unlike the liquid drop model, employs from the outset one of the stock models of quantum mechanics, hence the kind of representation of the nucleus is of a highly idealised nature. It also makes almost as good predictions as the liquid drop model. Yet, in the short period of rivalry between the two models (late 1930’s and early 1940’s) preference is shown towards the liquid drop model,¹⁰⁰ despite the fact (noted in the previous section) that it does not utilise a quantum mechanical description of the collective motion of the nucleus. I can discern three reasons for this. The first is that the main goal in the early days of nuclear physics was to find a model that justified the Weizsäcker formula. The Fermi gas model is set up in what we would

¹⁰⁰ See for instance Bohr 1936 and Bohr and Wheeler 1939.

generally regard as a theoretically systematic (or principled) way, based on bridge principles of quantum mechanics. That is, we have a description of the nuclear structure for which a quantum mechanical bridge principle exists that licences the use of a semantic model. In addition, the Fermi gas model predicts acceptably well, but it accounts for the terms of the Weizsäcker formula via semi-phenomenological considerations. The liquid drop model, on the other hand, is set up using a mixture of classical and quantum principles, i.e. not in any theoretically systematic way, yet it accounts for the Weizsäcker formula -to a tolerable extent- from first principles. The Weizsäcker formula, in other words, plays the role of a heuristic guide in the search of an appropriate representation model.

The second reason is that there exist two conflicting hypotheses, the strong interaction and the independent particle views, and the first one is the prevalent despite the fact that the second can accommodate quantum mechanical features more naturally. Hence, another element in the heuristic is the underlying hypothesis that is compatible with the existing empirical knowledge. The third reason, which is interconnected with the second, is that the explanatory power of the liquid drop, and in particular its assistance in understanding the phenomenon of nuclear fission, is valued highly; indicating that explanatory power also plays an operative role in the choice of the model.

What I choose to draw from this historical episode is a two-fold message. Firstly, what determines the application of a theory to a new domain of phenomena is the conglomeration of all background theoretical knowledge in conjunction with the existing empirical knowledge in the specific domain. The Weizsäcker formula is such a blend of background and empirical knowledge, for example it dictates that there should be a Coulomb contribution to the energy, also a volume contribution, and so forth. As a consequence the prevailing theory, quantum mechanics, plays a lesser role in the choice of the representation model simply because its own stock models don't fit. In other words, there do not exist any bridge principles that can tie the description of the nucleus, given primarily via the Weizsäcker formula, to one of the stock models of quantum mechanics. Secondly, the reason the strong interaction hypothesis is more widespread than its rival is because the empirical facts about the large-

strength and short-range nuclear forces impel the physics community in this direction. The stock models of quantum mechanics also play a lesser role to this outcome-choice.

The conclusion I choose to draw is this: in order to extend the scope of the theory to cover the nuclear domain, i.e. in order to apply the theory to a physical domain, we must construct representation models. To achieve this, we can start from a stock model and add corrections, which is usually the case when a particular physical domain has been extensively studied and such corrections are available, as in the case of the simple pendulum examined in Chapter 4. But if such correction factors to the Hamiltonian operator are not available, then the measure of the model's success is not a numerical match (of any acceptable degree) of its predictions to the experimental measurements, but whether or not it provides an acceptable degree of qualitative physical insight into the specific domain. In such cases, the theory gets applied via other background knowledge. In the example of the liquid drop model the theory gets applied via a model of classical mechanics, whose quantities get assigned quantum mechanical properties after the model is set up. It is true that we can use our background knowledge to construct a representation model that is entirely independent of the theory and then import that model into the theory, because it is something we do. But we must recognise that importing the model into our theory is a heuristic move of arbitrary nature, i.e. a move that is not licensed by a set of rules provided by the theory. In the liquid drop model example, to make the move from equation (5.9) to equation (5.10) we need a set of rules (more or less functioning like bridge principles) by which to convert the coefficients $\alpha_{\lambda\mu}$ to their canonically conjugate generalised momenta $\pi_{\lambda\mu}$. Such rules are not available. What we do is initially assume that the $\alpha_{\lambda\mu}$ play the role of coordinates, the $\dot{\alpha}_{\lambda\mu}$ that of velocities of the oscillators, and B_λ is given the role of mass. Then we make the arbitrary heuristic transition to the quantum mechanical domain by conjecturing that the $\dot{\alpha}_{\lambda\mu}$ are quantum mechanical operators and use a quantum mechanical converting relation:

$$\pi_{\lambda\mu} = \frac{\partial(KE)}{\partial \dot{\alpha}_{\lambda\mu}} = B_\lambda \dot{\alpha}_{\lambda\mu} \quad (5.12)$$

The canonical momenta are then introduced as quantum mechanical operators, which are assigned the usual commutation rules with the coefficients $\alpha_{\lambda\mu}$. The point is this, very often in physics the Hamiltonian is constructed with, more or less, a classical picture in mind. The quantum mechanical properties are then assigned to the classical Hamilton function (or the classical equation of motion) to obtain the quantized form of the classical picture. In our example, mathematical convenience or intuitive appeal are not the reasons for this mode of application of quantum theory. There is no clear quantum mechanical description of the collective modes of nuclear motion, hence we lack ways by which to assign a quantum mechanical stock model to this physical system. Instead, the terms of the classical Hamilton function get mapped onto their quantum mechanical analogues by means of ‘invisible rules’ ostensibly provided by quantum theory. However, these ‘rules’, on which the application of quantum mechanics rests, are not part and parcel of quantum theory.

This second way of expanding the domain of application of quantum theory is what stimulates caution when we identify a theory, following the SV, with a mathematical structure. If the theory does not supply us with the tools and principles by which to apply it in a particular domain, not because the physical domain is not believed to have quantum mechanical features but because the stock models of quantum theory do not fit that domain, and yet we still apply it successfully then I take it that there must be more to the theory than just structure. Furthermore, if we import an otherwise ‘alien’ model into the theory, then representation models cannot be simply considered extensions of the theory. Their predictive and explanatory successes, and not how they relate to the theory, determine the final verdict of representation models, such as the liquid drop. If the model gives reliable predictions and good explanations then we adopt it, otherwise we discard it altogether and try another. When a model is imported into the theory in the arbitrary manner I have described then its predictive and explanatory success is the only criterion we have for its acceptance. If we can use a stock model of the theory to represent a physical system, such as the Fermi gas model, then it is to our best. But how often does this happen, and what assumptions are involved for this to happen?

5.2.2.2 The Single Particle Shell Model

The single particle shell model of nuclear structure is generally viewed by nuclear physicists as an improvement over the Fermi gas model. Yet it is not the kind of improvement where we add correction factors to the initial Hamiltonian operator as to account more accurately for the factors that influence the physical system. Indeed, an additional hypothesis is made, which places the shell model onto a different category from its predecessor. This hypothesis involves an analogy with the physics of the atom, and in particular the orbital structure of electrons in complex atoms, which of course, is known to consist of allowed electron orbits that correspond to shells of a given value for the principal quantum number, with each shell having degenerate sub-shells specified by the orbital angular momentum quantum numbers. In the nuclear analogy, the nucleons are assumed to move approximately independently, in spite of the strong interactions known to exist between free nucleons, in specified shells (this last qualification distinguishes the shell model from the Fermi gas model).

The single particle shell model is the most primitive version of the family of shell-models. To make the principal presupposition behind this model more precise, assume that for an odd- A nucleus the nucleons are regarded as filling the shells (stationary orbits) in such a way that all of them except the last odd nucleon pair-off to form an inert core. This core is further assumed not to contribute at all to the angular momentum or the electromagnetic moments of the nucleus. Thus, the nuclear picture we are faced with is that of the remaining odd nucleon acted upon by the rest of the nucleus via a prescribed potential. Notice however that unlike the atomic case, in the nuclear case there is no central field produced by an external source (as the nucleus of an atom presents a Coulomb field for the orbiting electrons); one has only the strong attractions between the nucleons. So a corollary to the above assumption is that we consider the motion of the odd nucleon in the nucleus, under the influence of all other nucleons of mass M , as motion in a spherically symmetric *fictitious* central field of force. This idealisation, or theoretical distortion if you like, is of course still an improvement over the Fermi gas model, for the potential energy operator can now be considered as a function of position. The main problem, however, is that this

effective mean potential within the nucleus is unknown and the main task for the physicist is to discover it, or more appropriately to construct one that represents the physical system and its mechanisms. In the early 1930's, when this model was developed, the potential energy part of the Hamiltonian operators was chosen from the list of stock potentials of quantum mechanics. One such postulated potential is the *infinite square well*:

$$V(r) = \begin{cases} -V_o, & \text{for, } r < R. \\ \infty, & \text{for, } r > R. \end{cases} \quad (5.13)$$

Another is that of the *infinite harmonic oscillator well*:

$$V(r) = -V_o + \frac{1}{2}M\omega^2 r^2 \quad (5.14)$$

For both of these well-known potentials there exist exact analytic solutions to the Schrödinger equation. Nevertheless, they bear a very distant resemblance to a real nucleus because, among quite a few other things, they do not provide the possibility of barrier penetration through tunnelling.¹⁰¹ Stock potentials such as a *finite square well* or a *finite square well with rounded edges*, which (as compared to the ones above) bear a closer resemblance to the realities of the nucleus, can only be solved numerically. Therefore, although they do indicate significant improvement in predictive success on several fronts, they offer no help in gaining qualitative insights from their solutions. To avoid unnecessary dwelling on the failure of all these models to produce the desired predictions, I will point out just one. All these models predict only some (and not all) of the magic numbers, and since this is one of the primary nuclear features that an independent particle model -in particular, one that assumes spherical symmetry- should account for, questions in regard to the reasons for this discrepancy are sound. It is experimentally known that nuclei with either a proton or a neutron number that coincides with the magic numbers do not possess electric quadrupole moments in the ground state. Quadrupole moments are defined such as to measure deviation of the nuclear density from spherical symmetry; zero quadrupole moment indicates a spherical nuclear shape and large quadrupole moments indicate large spherical asymmetry. Since these shell models assume spherical symmetry it is expected that they should demonstrate zero quadrupole moments and hence at least

¹⁰¹ The transmission of energy even though the energy lies below the top of the barrier. This is a wave phenomenon and in quantum mechanics it is also exhibited by particles.

predict the magic numbers. But in addition to the predictive discrepancies, no one expects the form of field generated by the nucleons to be that of a harmonic oscillator or an infinite square well. The stock models of quantum mechanics offered no help on this issue, and for over a decade the single particle shell model was virtually abandoned and favour shifted towards the liquid drop model, primarily because of the need for an argument of physical significance in its favour.

The extravagant idealisations in which the single particle shell model is immersed, as well as its forerunner the Fermi gas model, were not the reason the two models were largely ignored for a number of years. On the one hand, the working scientists failed to discover convincing corrections to the potential energy operator that would increase the predictive success of the model. But also, on the other hand, at the time when the models were proposed, the climate quickly became unfavourable towards independent particle models predominantly for three reasons: (1) Because of the accumulating experimental knowledge of the large-strength and short-ranged nucleon interactions. (2) Because of the incongruous implication (of the shell model) for the presence of a common centre of force and the lack of available ways by which to circumvent this idealisation. (3) Because of the discovery of nuclear fission and the demand for an explanation of the phenomenon. Subsequently, the assumption that nucleons move approximately independently filling up shells in the nucleus was considered physically unjustified. Therefore, both insufficient predictive success and lack of explanatory power played a role in this outcome.¹⁰²

¹⁰² Despite the *ad hoc* way by which the liquid drop model is constructed, it seems apparent that it is preferred to the expense of not only the Fermi gas model but also the more sophisticated single particle shell model, due to its explanatory and predictive success and due to its capacity to account for the Weizsäcker formula from first principles. I do not think it is necessary to reiterate the previous argument.

5.3 More ‘Realistic’ Potentials for the Single Particle Shell Model: The Quest for Explanatory Power

The revival of the shell model research program had to wait for a breakthrough, which did not come until 1948. It was mentioned in the previous section that nuclei with either Z or N coinciding with one of the magic numbers should have zero quadrupole moments. Therefore, such nuclei should have spherical charge distributions. In addition, it is known experimentally that such nuclei have a relatively large binding energy per nucleon, i.e. the energy necessary to remove a nucleon from the nucleus is relatively large. Within the framework of the shell model, both of these empirical facts indicate that the magic numbers correspond to closed (filled-up) shells. Since the stock Hamiltonians of quantum mechanics (such as the harmonic oscillator and square well potentials) are extremely schematic single particle shell potentials, the Hamiltonian operator associated with the model (like so often in Physics) had to be constructed phenomenologically in order to reproduce *inter alia* the magic numbers.

In 1948 Mayer (and independently in 1949, Haxel, Jensen and Suess) suggested that a non-central force term be added to the central force potential of the shell model. This term was based on an *ad hoc* hypothesis, which postulated that there is an interaction between the orbital and spin angular momenta of the unpaired nucleon. This hypothesis, which revived the shell model from obscurity, implied that the potential energy operator should take the following form:

$$V(\mathbf{r}, \mathbf{l}, \mathbf{s}) = V(\mathbf{r}) + \frac{2\alpha}{\hbar^2} (\mathbf{l} \cdot \mathbf{s}) \quad (5.15)$$

I present the proposed potential operator above in such a form that any operator can be used for the central potential $V(\mathbf{r})$, and for the spin-orbit coupling term the value of the constant α could be considered as having no radial dependence. The spin-orbit potential is generally considered phenomenological because it is an *ad hoc* conjecture. The mechanism of the interaction was unknown. It derived its usage from the physics of the atom, but in the latter case the spin-orbit coupling of an electron bound in an atom arises from the interaction of the magnetic moments associated

with the angular momenta. If an electron moves in an electric field with certain velocity, a magnetic field is induced at its location. This induced magnetic field has an effect on the magnetic moment associated with the electron spin. From elementary electromagnetism it can then be shown that the magnetic moment is measured through its interaction energy with an external homogeneous magnetic field. That is to say, we know that the operator representing the interaction energy is given by the scalar product of the magnetic moment with the induced magnetic field. The magnetic moment is a function of the spin of the electron, and the magnetic field can be shown to be a function of the orbital angular momentum. Consequently, the interaction energy of the electron can be expressed as a spin-orbit coupling (i.e. a scalar product of the orbital and spin angular momenta). For an electron bound in an atom we thus have a theoretically regulated way of showing that there is an energy part of the Hamiltonian operator that is associated with the spin-orbit coupling.¹⁰³ For the nucleons, however, the spin-orbit coupling cannot arise from an induced magnetic field, because neutrons are uncharged particles and thus their motion does not give rise to magnetic fields. The mechanism of the spin-orbit interaction of nucleons is therefore largely unknown.

What Mayer (1948) did was to introduce a dimensionless constant (represented here as α) and change the algebraic sign on the spin-orbit coupling operator used in the atomic case. These changes imply that the mechanism which gives rise to the nucleon spin-orbit coupling is different from the electromagnetic mechanism (described above) that operates for electrons in an atom. The spin-orbit coupling was introduced into the potential energy operator of the shell model in an *ad hoc* manner, simply to adjust the model's predictions and thus account for empirical observations. The changes in the model predictions come about very simply and conveniently. Assuming, for the sake of brevity, that we choose $V(\mathbf{r})$ to be the harmonic oscillator potential, if α is chosen to have no radial dependence then the presence of the spin-

¹⁰³ The mathematical formalities for this mechanism, which are relatively straightforward, can be found in almost all books on atomic physics. I avoid them here for purposes of simplicity of exposition, by keeping this work mathematically 'clean' from physics not directly related to nuclear phenomena.

orbit coupling term does not change the harmonic oscillator eigenfunctions. Hence, the form of the solutions to the Schrödinger equation are just as simple as solving for an oscillator potential. Only the energy eigenvalues are changed by additive terms on l and j , where j is the total angular momentum. To be more precise, each energy eigenvalue of the harmonic oscillator is split into two parts. If α is positive, the energy for states of l anti-parallel to s are shifted to higher energies, and those of l parallel to s are shifted to lower energies. The addition of the spin-orbit coupling, as presented above, makes the empirically known magic numbers occur as closed subshells. To make them appear as clearly separate closed major shells without moving beyond this model, we can either adjust the α -values from shell to shell (i.e. give a radial dependence to α), or make the oscillator potential more square. The latter could be achieved by interpolating a potential $V(r)$ between the two extreme cases of the harmonic oscillator and the infinite square well.¹⁰⁴ The radial part of the wave functions of such an intermediate potential cannot however be given explicitly. This computational disadvantage can be overcome in a variety of ways by constructing more ‘realistic’ potentials that tend to lower the energies of the states with higher orbital angular momentum.

One such construction that achieves the above effect (firstly suggested by Woods and Saxon, 1954) uses for the $V(r)$ term in equation (5.15) an operator that was originally proposed for adjustment of the nuclear scattering potential in the optical model.¹⁰⁵ In this operator, r_n is the nuclear radius, which can be taken to be equal to the radial

¹⁰⁴ The reason for doing this is because for large l the infinite square well shells are energetically too low and the corresponding harmonic oscillator shells are too high relative to the observed values. This is generally understood as taking place because for large l the angular momentum barrier causes the nucleon to spend most of its time in the region near the edge of the well. Thus, the nucleon experiences too deep a potential in the square well and too little attractive force when a harmonic oscillator potential of the same depth as the square well is used. An interpolated intermediate potential can give the appropriate depth at larger distances for the states of large orbital angular momentum.

¹⁰⁵ The search for such potentials dominated much work on the independent particle models with shell structure in the late 1950’s and early 1960’s. In addition to the Woods-Saxon potential discussed here, other possible candidates were the Green-Wyatt potential, the Gaussian potential, the Yukawa potential. See Green *et al* (1968), Eisenberg and Greiner (1970), Von Buttlar (1968).

distance at which the absolute value of the potential drops to one half of its central value; and t is the surface thickness (or diffuseness of the nuclear surface, if one is considering nuclear scattering) of the potential. Both of these quantities contribute to the magnitude of the central potential:

$$V(\mathbf{r}) = -V_o \left(1 + e^{(r-r_n)/t}\right)^{-1} \quad (5.16)$$

In another approach to the problem of achieving the above effect, suggested by Nilsson in 1955, the Woods-Saxon potential is simulated by adding a term proportional to I^2 to the harmonic oscillator with spin-orbit splitting model:

$$V(\mathbf{r}, \mathbf{l}, \mathbf{s}) = \frac{1}{2}M\omega^2 r^2 + A\mathbf{l} \cdot \mathbf{s} + B\mathbf{l}^2 \quad (5.17)$$

The constant $A = -2\alpha/\hbar^2$, as before, characterises the strength of the spin-orbit coupling; and the parameter B simulates the deviation of the oscillator potential from a more ‘realistic’ potential (such as the Woods-Saxon potential). With this adjustment, however, the centre of gravity of the oscillator shells is not conserved and the model parameters must be readjusted for various shells. More recently, in 1967, Gustafson circumvented this problem by modifying the I^2 term to include the deviation of the I^2 operator from its mean value in the N -shell rather than the operator itself. Thus resulting in the following potential operator:

$$V(\mathbf{r}, \mathbf{l}, \mathbf{s}) = \frac{1}{2}M\omega^2 r^2 + A\mathbf{l} \cdot \mathbf{s} + B\left(\mathbf{l}^2 - \langle N | \mathbf{l}^2 | N \rangle\right) \quad (5.18)$$

There are a number of instructive issues, in relation to the construction of the single particle shell model with spin-orbit coupling, so far described, that I wish to take up, before moving further into the development of the most sophisticated version of the model, whose synthesis has to wait until Chapter 6.

5.3.1 Scientific Theories: Hypotheses vs. Mathematical Structures

Let me begin by rehearsing what I believe to be a plausible more accurate scenario for the SV in the story of the shell model, than the one alluded to so far, i.e. that we apply the theory by simply running through its semantic models to find one that matches the physical system. In the framework of the SV the nuclear shell model

would not be regarded as a single mathematical structure but as a structure-type, what we have called in sub-section 3.2.1.1 a state-space type. As a reminder, a state-space type is an abstract structure with some parameters left unspecified. Each time the unspecified parameters are specified in a particular way, the structure-type gives rise to one specific theoretical model. Hence a state-space type designates a family of models. What physicists call the single particle shell model is in fact such a family of models, of the structure-type associated with the Hamiltonian operator $H = T + V(\mathbf{r})$, together with the additional constraint imposed by the hypothesis that nucleons move approximately independently in specified nuclear shells and pair-off to form an inert core. When the kinetic and potential energy operators are specified then we have a fully determinate model. Of course, to make a just case for the SV, when new parameters are postulated (of which the potential energy operator is a function) we could view it as a new structure-type candidate for representing the physical domain of inquiry namely the one associated with the new Hamiltonian operator, e.g. $H = T + V(\mathbf{r}, \mathbf{s}, \mathbf{l})$, or simply as a structure-type nested in the previous one. Therefore, what we have earlier called the single particle shell model with spin-orbit coupling could be viewed as a new family of models proposed for the plausible representation of the nuclear structure, or as a structure nested in the family of single particle shell models. It would therefore seem that the application of a theory, according to the SV, is not as simplistic as I may have so far indicated.

Admittedly, this was a highly schematic way of describing what a theory application consists of according to the SV. The more detailed description implies that by imposing further constraints on the theory-structure, via peculiar-to-the-domain hypotheses, we define new structure-types and this results in a proliferation and nesting of theoretical models. This kind of analysis of theory application, suggests a process that runs all the way down to the representation (or concrete) model, to which I have attributed special status (wrongly, of course, if we abide to the SV analysis). I have stressed this latter point about the SV repeatedly throughout this work, because if its only contribution to understanding what scientific theories are were just the claim that (for example) the Schrödinger equation with a Hamiltonian of the general form $H = T + V(\mathbf{r})$ is a mathematical structure-type, then it would be no more than a

trivial contribution. The arguable implication of the SV, however, is that it considers representation models as descendants, i.e. natural (deductive) extensions, of the initial theory structure. This apprehension reduces the *actual* models we use for representation purposes to nothing else but the *pragmatic* outcomes of the efforts to stretch the theory all the way down to the phenomena, or to structures that relate approximately to corresponding theoretical models. For reasons that I will try to explain by using the shell model as an example, such oversight to the processes of construction of representation models and to theory application in general is, in my view, analogous to reading Tolstoy's *War and Peace* for the sole purpose of familiarising oneself with the historical events depicted in this epic masterpiece.

The construction of the single particle shell model relies on the general hypothesis of independent particle motion. The generic nature of this hypothesis allows for numerous mathematical manifestations (each of which is incompatible with the next, e.g. the Fermi gas model *vis-à-vis* motion of nucleons influenced by a parabolic potential). In order to use the hypothesis to construct a representation model we must introduce several addenda to it. It is further conjectured that the nucleons fill-up shells in analogy with the atomic case, and further that the nucleons pair-off to form an inert core, and that there is a spin-orbit coupling associated with the odd-nucleon that affects the latter's motion. The process of introducing addenda can go on for as long as our epistemic limitations allow. The result can be viewed as a conjunction of hypotheses or as just a more specific form of our initial hypothesis, and it has a mathematical manifestation with a more restricted scope to the previous one in the chain. The SV puts this story upside down. It claims that the laws of the theory define an infinite class of structures. Then, if we choose the structure -which we identify here as the single particle model with spin-orbit coupling- for representing the nucleus, then we invoke the theoretical hypothesis that the -data model about the nucleus is isomorphic to this structure. It is my understanding that in a more explicit form, the theoretical hypothesis would state that the nucleus is (isomorphic to) a system of independent particles that fill-up shells (stationary orbits) and in which the nucleons pair-off to form an inert core and finally that the spin-orbit interaction of the

odd nucleon also affects the latter's motion.¹⁰⁶ We may ask, does it matter that the SV gives us the wrong order of understanding scientific discourse? I think yes. If the SV gives us the correctly ordered picture of understanding scientific discourse, not much else can be said about scientific theories except to work out the details of this view. If however it does not, then we can justifiably infer that it is simply a rational reconstruction of the (actual thing) discourse that attempts to explicate the finished product, i.e. the theory. If we can find a persuasive argument for the latter position, then we can proceed to reveal what aspects of actual scientific theorising are obscured by the SV.

The SV places structures on a primary and hypotheses on a secondary level. Scientific hypotheses are reduced, according to the SV, to claims about the relation of a structure to a model of data. What is presumably puzzling in this picture is the question of how the structures are identified. Trivially they are the products of the scientific mind, but so too are a remarkably large number of other things, hence we cannot rest upon such a triviality. A reasonably careful examination of the SV leads one to infer that structures are defined 'objects'. This is, of course, substantiated by the explicitness of some proponents of the SV on this issue. In my examination of the SV in Chapter 3, I have tried to show that the means by which structures are defined, according to the SV, are what we generally call the laws of the theory. But if this is the case then the laws of the theory could be more fundamental than those objects they purportedly define, *viz.* the structures. Then the immediate question is, what sort of 'objects' are the laws of the theory. The most common answer to this has always been that the laws are hypotheses (one of the modes of these hypotheses, i.e. a way they are, is expressions of 'relations' that satisfy a structure). But then, if we have established that a small number of hypotheses (the laws) are what give rise to structures then we are surely sanctioned to extend this line of thought to other more

¹⁰⁶ It goes without saying that if any detail in the chosen model is changed it immediately implies a change in the explicit form of the theoretical hypothesis. No matter how much numerous philosophers hate to admit, whose intuition about scientific theories inhibits them from espousing the SV, the last thing we can charge the SV, as a way by which to comprehend what scientific theories are, is lack of coherence. To my knowledge, the number of interesting mistakes or shortcomings of the SV that have been pointed out are limited and have confined magnitude.

complex forms of hypotheses. It does not require much imagination to view any mathematical structure as an object determined by some complex hypothesis. Here is a sketchy example of such a complex hypothesis: the hypothesis that finds its expression in the Schrödinger equation (i.e. the laws of quantum mechanics) in conjunction with the hypothesis that nucleons are moving approximately independently, in conjunction with the hypothesis that nucleons fill-up shells, in conjunction with the hypothesis that nucleons pair-off to form an inert core, and finally in conjunction with the hypothesis that there is a spin-orbit coupling that also affects the motion of the odd nucleon. This hypothesis is used to define the structure that is commonly known as the single particle nuclear shell model with spin-orbit coupling. It would therefore seem that the SV gives us the wrong order by which to understand the overall picture. This may be the case because we could discover details about the character of models, and subsequently the character of theories, that are discernible only in a close study of the hypotheses that give rise to them, and that are otherwise obscured. The fact that hypotheses are the defining tools of structures gives us enough grounds to proceed in this direction. I do not perceive this as a conclusive argument against the SV, but more as a reason why I believe that the SV is just a rational reconstruction of theories.¹⁰⁷ As such, by concentrating on the derivative structures for the analysis of theories, and by omitting the role of the structure-defining hypotheses, it is plausible that the SV errs on three fronts. (1) It distorts elements that constitute actual scientific theorising -and in particular the scientific activity of theory application- by reducing the latter to more or less a pure mathematical activity. (2) It fails to recognise major scientific quests, such as the search for explanatory mechanisms. (3) And finally, it presupposes a mistaken view of how idealisation and its converse process, de-idealisation, is practised in scientific inquiry. All three of these shortcomings are intertwined with my general view that representation models in science are not what the SV wants us to understand. The first of these is a criticism of mine that is present throughout the entire of this work. The second I address shortly in the present sub-section and continue in sub-section

¹⁰⁷ I wish to remind the reader that by a *rational reconstruction* of theories I mean the suggestion of a logical framework into which theories can be essentially reformulated, i.e. an *explication* of the structure of theories and not a *description* of how actual theories are formulated.

5.3.3, and the third I will address in sub-section 5.3.2 that follows and will return to in Chapter 6.

But we have also just discovered something else about the SV: namely, that it attributes two functions to hypotheses. Firstly, they are used to define structures, and secondly they are used to make truth-claims about the structural relation between the mathematical structure and the data model. But these two functions render the structures, from a certain point of view, redundant -although sometimes convenient-mediators between the defining hypothesis and the experimental data. In what sense are they redundant? The idea that hypotheses define structures could be interpreted, as a way of stressing the view that theories do not make direct claims about the phenomena *per se*, as it has been traditionally understood. Scientific hypotheses make claims only about idealised and abstract systems (i.e. about idealised and abstract sets of objects subject to a certain set of relations). What I am suggesting here is that the primary difference between the Received View and the Semantic View be understood as follows: in the former, theoretical claims have traditionally been understood to refer to statements about actual phenomena; whereas in the latter, theoretical claims should be recognised to refer to abstract and idealised structural ‘images’ of the phenomena. In the Logical Positivist program on scientific theories we begin with a set of hypotheses (the axioms) and establish a deductively closed system that stretches all the way to truth-functional statements about observations. Of course, there are further complications to such a view and its proponents have supplemented it with correspondence rules and the like to overcome them. The point however is that in that view it became prevalent, but not pervasive (a notable exception is Hempel 1988, but also the physicalist program of Neurath that the young Hempel seemed to have shared -see Friedman, unpublished manuscript), that the theoretical statements refer to actual empirical systems. The SV combats the Logical Positivist syntactically motivated attitude by utilising mathematical structures. The hypotheses define structures and are then used to make claims about the relation of these structures to their physical counterparts (the data models). Among other things, the purpose achieved is to emphasise that the laws of the theory do not refer to actual physical systems, i.e. strictly speaking they have no factual content. Although arguable, this point is well taken, but we need not embrace the SV and all its implications in order

to adopt this position or some variant of it. If the SV is embraced, then its focus on structural analysis and its demand that every scientific model be subsumed in the theory structure comes at certain costs: (1) the explanatory nature and partial theory independence of representation models is concealed, and (2) the process of idealisation and its converse, as a process of theory application, is given a far too simplistic and inadequate account.¹⁰⁸

When we want to check for the connection between representation models and the theory that gives rise to them, the above line of reasoning becomes important. The SV holds that there is unity of all models of the theory, a claim that is justified by subsuming every model (including representation models) to the theory structure. It then makes use of structural analysis, which ultimately leads the proponents of the SV to resort to the claim that the actual model used for representation purposes relates in some approximate way to a model that belongs to the theory structure.¹⁰⁹ A more farfetched version of this view, that may not be shared by all the proponents of the SV, is to regard representation models as the pragmatic outcomes of the attempts to stretch the theory to the phenomena. That is, to the position that representation models are constructed on purely pragmatic grounds. If however we step outside of the structural analysis of the SV and rely on an analysis of the hypotheses that give rise to the structure, it becomes clearly difficult to defend this position. Indeed, in the case of the shell model, what is evident is that the choice of the hypotheses is not taken on pragmatic grounds but it is based on the search for (explanatory) mechanisms that may influence the physical system of concern. In addition, it becomes easier to understand why at different stages in the construction of the representation model, existing stock models of the theory are invoked to represent a

¹⁰⁸ If the claim about the redundancy of structures is valid then we may also infer, that by focussing on the structural character of theories the SV aims to explicate (and justify) the unification of theoretical claims. Viewing the theory as a class of mathematical structures ensures the unification of theoretical claims. Moreover, we can see that despite the fact that the SV was partly intended to disrepute the logical positivist distinction between the context of discovery and the context of justification it nevertheless maintains its own version of such a distinction, camouflaged within the additional subtlety that the act of ‘discovery’ is now viewed as the act of ‘construction’.

¹⁰⁹ In Chapter 4, I have argued that this claim is unfounded.

particular mechanism or on some occasions new suitable stock models are constructed. That is to say, we can understand that it is not just the theory that gives birth to representation models, but also that representation models exert their effect on the theory by instigating an expansion of its scope.

Take, for example, the shell model where we initially begin with the hypothesis that nucleons occupy shells, which implies a central potential. The stock models of quantum mechanics are tried out as mathematical representations of this potential, e.g. the harmonic oscillator or the infinite square well, because our initial hypothesis allows that we start from the available stock models (contrary to the strong interaction hypothesis). The central potential hypothesis has a heuristic nature, some of its initial elements will be retained and some others will eventually be discarded or modified. The specific stock models we try out, however, are based on several subsidiary working hypotheses, because the goal at this stage is not to discover the final acceptable potential operator from among the stock potentials of the theory (unless coincidentally we happen to stumble upon it). The purpose of these working hypotheses is to find the most suitable *initiatory* mathematical representation of the central potential that will enable the exploration of the nuclear domain. That is why we should regard them as working hypotheses, to be tried out and be discarded once they achieve their purpose. Physicists know very well from the beginning that even if they finally adopt some modified version of the shell model for representing the nuclear structure, the final potential should have deviated significantly from the central potential. They know this because the physics of the nucleus cannot accommodate a picture based on a central force. So by trying out the different stock models the primary purpose is not what it may seem, i.e. fitting the experimental facts, but it is to discover how the central potential should be modified. They are searching, in other words, for mechanisms that would explain the desired shifts of the model predictions from that of the central potential. The search for such mechanisms is the key to understanding the shell model.

The spin-orbit coupling associated with the odd-nucleon is such a postulated *ad hoc* mechanism. The spin-orbit hypothesis adds its own character to the heuristic. It gives the wanted *kinds* of modifications to the model predictions and hence it is a starting

point; it guides the research community to search for better stock models (for the central part of the potential) that would give more refined predictions (e.g. the Woods-Saxon potential). It is very frequently taken for granted that the models of a theory are available for modelling a domain the moment the general theory structure is defined. So far I have disputed the availability of a ready-made, or antecedently available, (infinite) class of models for the theoretical representation of physical systems, by pointing to the work of Cartwright (1983 and 1999) and the qualification which she makes that the theory only supplies us with a finite set of stock models for such a purpose. Now I want to further qualify the argument. Part of scientific theorising consists in the construction of new stock models that may be of use in a particular domain. The motivation for their construction comes from the demand of the mechanisms at work in the representation model. This point is manifested in the case of the Woods-Saxon potential (as well as other proposed potentials). The Woods-Saxon potential is constructed because we want to pursue the spin-orbit hypothesis, it provides a better defence for the latter because together they improve on the predictive power of the model. To make the point more general, the stock models of the theory employed in the construction of a representation model (in a particular domain) are partially determined (or regulated) by the postulated mechanisms at work.¹¹⁰ I take this point to rebut the position that representation models are constructed on purely pragmatic grounds, because if such were the case then representation models would just be constructed by the addition of correction factors to the initial stock model. In the case of the single particle shell model what determines the mathematical representation of the potential is the interplay between theory and the auxiliary postulated mechanism of spin-orbit coupling.

It is interesting to contrast the introduction of the Woods-Saxon potential to the introduction of the third terms $B\mathbf{l}^2$ and $B\left(\mathbf{l}^2 - \langle N|\mathbf{l}^2|N\rangle\right)$ in equations (5.17) and (5.18) respectively. These terms are not the outcomes of hypotheses that postulate new mechanisms like the spin-orbit coupling. They are introduced for purely

¹¹⁰ This same point can also be made for the construction of the optical model of nuclear scattering, for which the Woods-Saxon potential was originally constructed.

pragmatic reasons. They are correction factors to the harmonic oscillator potential to make it simulate the Woods-Saxon potential. I pointed out these cases because I want to emphasise that the addition of correction factors to an initially chosen potential energy operator occurs when we have a description of the physical system that validates the adoption of the chosen operator. When this situation occurs, then we may have enough grounds to support the thesis that actual representation models are constructed on purely pragmatic grounds. This however is a situation that we generally encounter in well-explored domains. With hindsight we come to regard every part, but the first, of our Hamiltonian operator (or force function etc.) to be a cumulative correction factor to our initial stock Hamiltonian. In the beginning of the exploration of a physical domain however, like in the case of nuclear physics, it becomes conspicuous that in the construction of a representation model what we eventually come to regard as correction factors are in fact postulated mechanisms at work that determine the choice of our stock model. And in many cases these postulated mechanisms impose the need for ‘mini-research programs’ to construct more suitable stock Hamiltonians and bridge principles to thus enrich the theory.

The SV would give us a different explanation for the foregoing story. It would claim that an actual representation model is a structure, part of which is included in the theory structure plus some extra structure for which the theory does not account. This extra structure that has to be ‘filled-in’ in order to match the corresponding data model, is what is added to the model of the theory on pragmatic grounds. This explanation, however, does not discriminate between those cases that, I have claimed, postulate novel mechanisms at work from those that do not. If we view the process of construction of a representation model solely as an activity that perpetually supplements structure then it seems inevitable that we conceal this feature. If however, we view this activity as the perpetual synthesis of hypotheses, as I suggest, then this feature becomes clear. When we view the final product, i.e. the representation model, without attending to its process of construction then, no doubt, the SV is correct in pointing out that it is a mathematical structure. Part of this structure may be subsumed in the theory that gave rise to it, and part of it may be alien to the theory. But this is only a rational reconstruction of the finished product,

which I take it to be equivalent to saying that any model can be traced back to the theory that gave rise to it because it shares structure with the latter.

If we come to regard, as suggested earlier, representation models as structures defined by some complex hypothesis, then we have an empirical criterion by which to decide what parts of the hypothesis-conjunction are of pragmatic nature and which are not. Any part of the conjunction that postulates a physical mechanism is subject to empirical investigation. Whereas, any conjunct that is only meant to provide a mathematical simulation cannot be tested empirically. The latter is only testable as part of the model, for it is added to the model for pragmatic reasons, solely to adjust the model predictions. The former however possesses the capacity for explanation, once enough experimental evidence is acquired for establishing the existence of the postulated mechanism. These postulated mechanisms are the elements of the models that provide the nexus between models and real physical systems. Compare this to what the SV has to say on the issue. A certain aspect of the model is of pragmatic nature or it relates approximately to the theoretical concepts, if it does not occupy a salient place in the unifying theory structure. But the pursuit to subsume all models under the umbrella of a unifying theory structure is motivated by the same exact *a priori* view as that held by the Logical Positivists, i.e. that the deductive consequences of the theory stretch all the way to the phenomena. Only, for the SV the justification for this position does not require resorting to an observation/theory distinction, but instead to a sharp distinction between models of theory and models of data. This distinction inevitably leads to the view that every actual representation model is constructed on purely pragmatic grounds or is an approximant of some model of the theory, thus concealing those elements of the models by which we may reasonably link them to physical systems.

5.3.2 Remarks on Idealisation: The Cumulative Correction Process

This discussion inevitably leads us to questions on idealisation and in particular what role it plays in theory application. The most familiar account on the issue is that of McMullin (1985). In his essay, McMullin maintains a view that comes very close to

equating idealisation with approximation. He contends that scientific theories are usually idealised descriptions that apply only to circumstances that can be achieved in the highly contained environments of laboratories, but seldom do they occur in real world environments. Focussing on the methodological dimension of McMullin's argument we can view the converse process to idealisation, i.e. de-idealisation, as a way by which a theory gets applied to the phenomena. We start from a set of highly idealised principles, to construct an initially idealised model. The latter can be further de-idealised by utilising other conceptual resources of the theory, which enable us to add correction factors to our initial model. Eventually we end-up with a reasonably de-idealised model that, in McMullin's realist outlook, approximates the real system.¹¹¹ In a more epistemologically neutral outlook it *resembles* the real system it ostensibly describes. To visualise the process we can think of the pendulum model explained in Chapter 4. The de-idealisation of the linear harmonic oscillator proceeds cumulatively by the inclusion of more and more influencing factors. In fact, the proponents of McMullin's views would claim that the pendulum example exemplifies their account of de-idealisation. Following my discussion of the pendulum however, I find such a claim highly arguable, given that the correction factors to the classical simple harmonic oscillator model very frequently do not belong to the conceptual resources of classical particle mechanics.

The peculiarly interesting thing about McMullin's account of the de-idealisation process is that it is remarkably useful for the proponents of the SV, realist and anti-realist alike. To see why, let's consider a general hypothetical case. Suppose we have an initial idealised model that is expressed in the following functional relation: $H(x) = f_1(g_1(x), \dots, g_n(x))$. Then, a first step de-idealisation would be to expand the functional relation by accounting for a physical parameter that (according to the

¹¹¹ Arguing from a realist perspective, McMullin (1985) proceeds to show that the idealised nature of theories presents no epistemological problems to the realist, because the theory possesses the necessary conceptual apparatus to de-idealise its models to the point where an approximate description of actual physical systems is achieved. I am not concerned with the epistemological dimension of McMullin's argument. Here I want to focus on the methodological dimension of how idealisation and in particular its converse, de-idealisation, can be used as an instrument to understand the application of theory to the world.

theory) we have initially ignored. The result would be a new functional relation: $H'(x) = f_1(g_1(x), \dots, g_n(x)) + f_2(h_1(x), \dots, h_m(x))$. The process of supplementing the function $H(x)$ with cumulative correction factors can go on, presumably until the theory has no more to offer on the issue. Clearly, all the derivative de-idealised relatives of our initial model relate to the latter in an obvious way: in the limit as the correction factors tend to zero they yield the initial model. Thus in our general hypothetical example above we can express the relation between the two models, as follows: $\lim_{f_2 \rightarrow 0} (H'(x) = f_1(g_1(x), \dots, g_n(x)) + f_2(h_1(x), \dots, h_m(x))) = H(x)$. In other words, on this account idealisation is the process by which we let factors, that according to the theory are influential to the physical system, tend to zero. De-idealisation is the converse process of allowing these theoretically dictated factors take finite values. This is what the processes of idealisation and de-idealisation consist of in McMullin's view. And this I claim, is the most suitable account of de-idealisation if it is to be accommodated as a process of theory application in the SV.

To see why let me view the process more abstractly. We know that, according to the SV, a model with some parameters unspecified is a defined structure-type. When a new parameter is specified we construct a new structure-type nested in the previous one. Imagine this process repeated for a number of parameters. The result is a sequence of structure nesting. Thinking about this abstractly, idealisation/de-idealisation is no more than a partial ordering of structures.¹¹² The criterion (i.e. relation) of this partial ordering is that of the restriction of the domain, i.e. two models are partially ordered (i.e. $M_1 \lesssim M_2$) if and only if the domain of M_1 is a restriction of the domain of M_2 (i.e. $\text{dom} M_1 \subseteq \text{dom} M_2$). Intuitively, we could think of the criterion for partial ordering as the specification (or addition) of a parameter in the functional relation.

¹¹² It makes no significant difference to this argument whether we talk of strong or weak partial ordering. Strong partial ordering requires that the relation satisfies the conditions of anti-symmetry and transitivity; whereas weak partial ordering requires the satisfaction of the conditions of reflexivity, weak anti-symmetry and transitivity.

But immediately we discern that if such is the case, then this de-idealisation view of applying a theory is part and parcel of the SV, whose formulation implies a partial ordering of structures. This holds for the views of realists like Suppe and anti-realists like van Fraassen. The difference between the two would lie in the character they would assign to the correction factors. The former would claim that the conceptual resources of the theory determine the correction factors but the final model is still an abstract and idealised replica of a real system. Primarily due to reasons attributed to tractability, or mathematical and numerical limitations, or simply the incapacity to extend the domain of the theory beyond a certain stage because the theory's conceptual resources have restricted scope. Van Fraassen would claim that some of these correction factors are theory-instigated, whereas others are introduced on purely pragmatic grounds, but all that matters is that the final model is structurally isomorphic to the respective data model.

In Chapter 4, I argued that the correction factors to the pendulum are not introduced by using the conceptual apparatus of the theory alone. Many other things are involved in the process of de-idealisation. In particular, we make use of auxiliary empirical laws and experimentally determined values for the associated parameters. Here I want to augment the argument and claim that it is quite explicit in the single particle shell model that supplementing the model with the spin-orbit 'correction factor' involves postulating a novel hypothesis. The specific hypothesis of the spin-orbit coupling postulates a mechanism that the theory offers no assistance in discovering. Hence the SV lacks the capacity to subsume the single particle shell model with spin-orbit coupling under the theory structure. The primary indication for this is that, for the reasons mentioned in the previous section, the introduction of the spin-orbit coupling hypothesis cannot be given an adequate explication by understanding de-idealisation as a process of theory application in the way explained above. But I also want to urge by this argument a more general claim: implicit in the SV is a far too simple and distorted account of idealisation/de-idealisation as a process of theory application. This account has two key features, (1) that the introduction of correction factors is cumulative and (2) that the correction factors are dictated by the conceptual resources of the scientific theory that gives rise to the initial model. The second feature fails to

account for the introduction of the spin-orbit coupling; the first feature as we shall see later (in Chapter 6) fails to account for the construction of the unified model.

5.3.3 Representation Models: Media for Scientific Explanation

In the particular case of Mayer's spin-orbit coupling hypothesis, it may be argued that the fact that the postulated mechanism is *ad hoc* renders the resulting model a pragmatic counterpart of a theoretical model. In contending this, however, we overlook the most important nature of the representation model: to borrow from Morrison (1999), its autonomy. Although there is no theoretically systematic way to justify the introduction of the spin-orbit force, as is the case for the atom, it does not mean that experimentally motivated arguments are disqualified from providing the justification. Indeed this is what takes place. Because of the piecemeal fashion by which the model is constructed, the concern with substantiating the spin-orbit force shifts to the experimental facts. Numerous scattering experiments were conducted some of which succeeded in showing that the spin-orbit force exists in nuclear matter. Examples are Adair (1952), Hensinkfeld and Freier (1952), Signell and Marshack (1957 and 1958). This is very similar to my claim in Chapter 4, that the closest representation models get to theoretical models is when the latter are blended with empirical ingredients. I used this argument to point to the fact that the distinction between theoretical and data models is not as clear-cut as the proponents of the SV want us to think. The same holds for the present case.

Empirical evidence in support for a postulated physical mechanism is usually acquired by the use of another mode of scientific inquiry, which does not involve the theory directly but is strongly associated with representation models. This mode of inquiry is peculiar to the representation model because when used together with experiment it is the way by which to render the model explanatory. It involves holding constant the important components of the model-defining hypothesis and altering some of its subsidiary parts, in an attempt to discover empirical evidence in support of a postulated mechanism. Indeed this is the case for the spin-orbit hypothesis, which can serve as an example by which to clarify this point. One of the

conjuncts in the defining hypothesis of the model (other than the spin-orbit coupling hypothesis) can be modified in ways that enable inferences to be made about the postulated spin-orbit mechanism. One of the hypotheses that underlies the single particle shell model involves the idealising assumption that the nucleons pair-off to form an inert core. If this hypothesis is modified (in the way explained below) and all other defining parts of the model remain equal, we define a model that I shall refer to as the *loose particle* shell model (as substitute for the clause ‘single particle’).¹¹³ The physical intuition behind this particular modification is that those nucleons that fill up shells form an inert core, and the loose nucleons that remain in the unfilled shells (whether odd or even in number) contribute to the nucleus’s properties. Although, the loose particle shell model (being less idealised) generally makes different predictions from the single particle shell model, its predictions about spin are the same as the latter’s,¹¹⁴ thus showing that the spin-orbit interaction makes a contribution to the nuclear behaviour. The initially *ad hoc* hypothesis of spin-orbit coupling was thus later to be elevated to the status of a prediction of a property of nuclear matter, despite still being in need of theoretical subsumption. It would be off the target to claim that some future Mesonic theory will provide the necessary explanation for the spin-orbit hypothesis and even the answers to, possibly the most haunting scientific question of, ‘what holds the nucleus together’. The claim urged in my argument is

¹¹³ There is the immediate tendency to call such cases de-idealisations or better approximations. I do not oppose the general position of such an apprehension but, given the preceding discussion in subsection 5.3.2, I choose to exercise caution because what is modified in the single particle shell model is one of the defining hypotheses. The implications of the modification vary in character and degree in ways that distinguish it from modifications that involve simple addenda to the Hamiltonian operator. One such example is that the explanatory power of the single particle shell model is extended in the loose particle shell model because the latter can supply us with an explanation of both odd-*A* and even-even nuclei. I shall try to elaborate more on the issue of de-idealisation shortly in Chapter 6, in an attempt to discern the elements of this process of theory application and their character. But what is at issue here, following the preceding discussion, is that an accurate account of idealisation/de-idealisation is still wanting.

¹¹⁴ I do not provide the experimental results and accompanying explications, but the reader is referred to any of the following nuclear physics literature, Preston and Bhaduri (1975), Segré (1977), Burcham (1973), Elliot and Lane (1957), and many text books on Nuclear Physics with an experimental touch to them.

that representation models are the means by which any theory gets applied to the world. The fact that representation models postulate theoretically unexplicated mechanisms, is part of the reason why new theoretical research programs are instigated. It goes without saying, that my argument is meant to claim that even our future Mesonic theories will be in need of their own representation models.

Morrison argues that one of the roles representation models have, in scientific activity and inquiry, is that of autonomous agents. 'The autonomy is the result of two components (1) the fact that models *function* in a way that is partially independent of theory and (2) in many cases they are *constructed* with a minimal reliance on high level theory. It is this partial independence in construction that ultimately gives rise to the functional independence.' [Morrison 1999, p43] I think that the 'bold' spin-orbit hypothesis as introduced into the shell model, and the ways by which its introduction is justified, is an example that supports Morrison's argument. Moreover, it supplies the model with explanatory power that derives from other sources and not from existing theory and it gives the model its (partial) representation capacity. Both of these interconnected features provide the model its special status as a mediator between the theory, which is by its own nature 'segregated' from experience, and the phenomena.

An important part of the reason why representation models enjoy a certain autonomy is because the subsidiary hypotheses that impose constraints on them, over and above those imposed by the theory, are not part of the theory for which the models mediate. These hypotheses differ in kind from those used in the definition of the stock models of the theory. The latter determine the scope of the theory: its stock models. The former however are attempts to explore physical domains. Their primary purpose is to extend the scope of the theory to particular physical domains. These additional hypotheses imply a particular nature to the subject matter of the model, which is in need of an explanation. The explanation can come from wherever we can find it, whether from theory-based or experiment-based considerations (or a mixture of the two) it does not matter, because the goal is to furnish explanatory power to an important medium of scientific explanation: the representation model.

5.4 Conclusion

I have argued in this Chapter that representation models cannot be subsumed under the theory's structure, as the SV suggests. They are autonomous agents of scientific inquiry because they possess explanatory and predictive power independent from that of the theory. They may be constructed by the use of background knowledge and semi-empirical results, as is the case of the liquid drop model. Or, they may be constructed by stock models of the theory that have to be amended by postulating novel mechanisms for which the theory offers no justification. Or, they may be constructed by a combination of the two methods, as we shall see in Chapter 6 for the case of the unified model. Whichever the case, the point is that this independence from theory in construction 'ultimately gives rise to their functional independence'. By functioning independently they provide us with explanations of physical systems for which otherwise we would be kept in the dark.

It is clear from the examples of representation models I provided that we do not apply quantum theory just by using its theoretical models but in a variety of ways. These ways suggest that the theory of quantum mechanics is much richer than just its class of semantic models and its small number of bridge principles. The failure of the Semantic View to capture the functions of representation models and to explicate their processes of construction is the clear message in the arguments presented in this chapter. But the overall argument continues into Chapter 6 in hope that we can provide an explication of the processes of construction of representation models, that does justice to the evolutionary history of representation models and to the physical content that model-defining hypotheses furnish the models.

6 The Unified Model of Nuclear Structure

6.1 Introduction

We have so far looked at the single particle shell model to which we later added the spin-orbit interaction. It was stressed that this model enjoyed a lot of success in correlating and predicting a large number of nuclear data, and that it displaced the liquid drop model for a short period of time. We specifically focused on the case of the magic numbers. But it should also be mentioned that the model enjoyed success in providing a relatively good understanding of the ground state spins and parities of most A -odd nuclei, and of many features of nuclear decay schemes. Despite its relative success the model was known to rest upon an idealisation for which there was room for improvement. One of the underlying assumptions, mentioned earlier, of the model is that the nucleons pair-off to form an inert core of spin 0. Indeed this kind of coupling is not far from that expected, in view of the large-strength and short-range attractive nature of nuclear interactions. But in fact actual nuclei exhibit inter-nucleon interactions that give rise to additional correlations. It was also briefly mentioned that this idealising assumption could be substituted with a more ‘realistic’ one that considers only the filled-up shells as the inert core of the nucleus.

If we compare equation (5.15) to the potential energy operator of equation (5.2) we realise that equation (5.15) approximates the independent particle hypothesis by imposing further constraints. In particular, the constraint that nucleons fill up shells

and pair-off to form an inert core or any alternative to it (such as, the loose particle shell model constraint). This, of course, is just one of many ways to proceed. I have so far neglected to mention that parallel to the development of the single particle shell model dealt with here, another distinct method of exploring shell structure was developed, in which the above assumption is side-stepped: the so-called Individual Particle Shell Model.¹¹⁵ The individual particle shell model can be understood as a model that assumes a generalised form of shell structure. In this case, the pairing-off constraint is completely dropped and inter-nucleon interactions are entirely accounted for and are considered the source of the nuclear potential. In other words, in the individual particle shell model we do not introduce an approximated form of the potential energy operator into equation (5.2), as in the single particle shell model case. Instead, the many-body nuclear problem, which as mentioned in the very beginning of the previous Chapter is insoluble, is dealt with directly accounting for pair-wise nucleon interactions and for influences exerted on them from the other nucleons. The inevitable consequence of this method is to resort to available variational techniques, such as Hartree-Fock field methods, for its solution. Thus in the single particle approach we encounter the case of approximating the Hamiltonian operator we wish to solve the Schrödinger equation for, and in the individual particle approach we use the exact operator that represents our hypothesis about the physical system and then solve it by approximation methods. Because of the complexity of the variational methods used in the individual particle shell model case, a lot of the underlying physics is obscured. Thus I chose to present the single particle shell model in which the elements that I believe are necessary in our quest to understand representation models are easier to discern. This personal choice, however, makes no difference to the overall argument because the same abstractive hypothesis underlies both of these models: the collective mode of nuclear behaviour is totally neglected. Subsequently, if the explanatory and predictive power of both models is to significantly increase, the wavefunctions of both models should be improved by the introduction of collective modes of motion. The introduction of collective modes of

¹¹⁵ In the nuclear physics literature, this model frequently goes by the name of 'Many Particle Shell Model'.

nuclear motion is of course largely inspired by the strong interaction hypothesis.¹¹⁶ The methods by which these improvements are introduced in the two models do not differ in any philosophically significant ways. That is why I have so far focused, and will continue to do so for the rest of this Chapter, on the single particle shell model and its development and improvement. The collective mode improvements on either of the two approaches to shell structure give rise to what is generally known as the Unified Model of nuclear structure.

6.1.1 Preliminary Remarks on the Background to the Unified Model

It became apparent in the early 1950's that the shell model research program could not furnish acceptable explanations for several nuclear phenomena, e.g. nuclear fission and the giant resonances of nuclei (a phenomenon first observed in 1947, to be discussed shortly). It also became apparent that the shell model's fictitious assumption of a central potential could not be adequately corrected by reliance solely on the independent particle hypothesis. In 1952 Bohr, and later in 1953 Bohr and Mottelson, developed a model for the collective modes of nuclear motion, which came to be known as the Collective Model of the nucleus. This model relies heavily on the liquid drop model (it could justifiably be viewed as a more sophisticated version of the latter) although some basic assumptions are modified. The collective model is, most probably, the most sophisticated strong interaction model. It considers the nucleus as a collection of closely coupled particles and by use of the hydrodynamic analogy, the Hamiltonian for the collective motion of the nucleons is developed. The collective Hamiltonian consists of four terms each accounting for the vibration, rotation, giant resonance, and a mixture of vibration-rotation modes of

¹¹⁶ This procedure could involve the introduction of the concepts of the liquid drop model, which is the line pursued in the present work. But for the sake of accuracy, it must be noted that the same can be achieved by using the concepts of another strong interaction model, the alpha particle model that was mentioned briefly in an earlier footnote.

collective nuclear motion. The collective model was an inspiring drive for a new direction to the shell structure research program.¹¹⁷

The outcome of this research direction was the unified model, which although it incorporates many of the peculiarities of the collective model it does so by assimilating these into the character of shell structure. Frequently in the nuclear physics literature, we encounter the view that the unification of the single particle shell model with spin-orbit coupling with the collective model is a form of synthesis of the two distinct models. I shall not pursue this line in my analysis; instead, I shall consider the unified model as an improvement, but not an extension, of the single particle shell model. My reasoning for this relies heavily on the model-defining-hypotheses view elaborated in the previous Chapter. The development of nuclear models began in the 1930's with two underlying conflicting hypotheses. One assumed that the motion of the nucleus is roughly equivalent to the motion of strongly coupled particles and the other assumed that the nuclear-constituent particles moved independently and that their motion is what constitutes aggregate nuclear motion. The first gave rise to the liquid drop, alpha particle and collective models and the second gave rise to the Fermi gas and the various kinds of shell structure models.¹¹⁸ These hypotheses were conflicting not by virtue of their logical incompatibility (this is why I chose not to call them contradictory or incompatible, despite such frequent characterisations in the physics literature), but by virtue of the underlying physical intuitions and the different quests for explanation of experimental knowledge. The first hypothesis is motivated by the perpetually accumulating experimental knowledge of the large-strength and short-range nuclear interactions, by its resulting models providing good explanations for phenomena such as nuclear fission and later on giant resonances, and finally for providing a good justification for the Weizsäcker formula. The second hypothesis is motivated primarily by the fact

¹¹⁷ A. Bohr 1952, A. Bohr and Mottelson 1953, (A. Bohr was the son of N. Bohr). Since the unified model is simply a linear synthesis of the concepts of the single particle shell model with spin-orbit coupling with those of the collective model, I consider it unnecessary here to discuss the latter on its own.

¹¹⁸ The development of nuclear scattering models demonstrates, in many respects, similar features.

that it makes use of quantum mechanical principles from the outset, and it gives rise to models that seem to be the result of quantum theory alone. The shortcomings of all offspring models of the above hypotheses were pointed out in Chapter 5. I think that the development of the unified model demonstrates an example in which the synthesis of existing physical intuitions takes place at the level of the defining hypothesis. Indeed, what seems to be a synthesis of two distinct models is in fact just the use of the existing mathematical representations of the different Hamiltonian terms, in the context of the unified model. This context however, which is dictated by the model's underlying defining hypothesis, implies an alteration in the physics of the predecessor models. If we view the unified model as a synthesis of the two previous models then we fail to see the changes that take place in its physics.

In the rest of this Chapter, I want to present the unified model of nuclear structure, and attempt to shed some light on its process of construction. In doing so I will be arguing that one important element of our scientific inquiry into the world is that progress in the construction of representation models in a particular physical domain relies, to a large extent, on the explanatory and predictive successes of predecessor models. I will try to show how, and argue why, the unified model relies on the success of the single particle shell and collective models. In effect, I will be arguing that the use of representation models is just as vital as theory in how physical knowledge about a particular physical domain is accumulated. I argue that the unified model of nuclear structure, which is the most sophisticated model of the nucleus that has been constructed, synthesises all our knowledge about the nucleus, gained by the explanatory and predictive successes of earlier models. The arguments of Chapter 5 that preceded the presentation of the unified model also hold for its case, this becomes clear in my presentation of its construction, hence there will be no need for detailed repetition.

In Section 6.3 I will present an account for the thought process that underlies the construction of the unified model. It is along the lines of existing work on idealisation, but I choose to call this process 'the process of abstraction and concretisation'. I believe that this account of the process captures the ambient factors involved in the construction of the unified model. My argument, however, is not

confined to the unified model. I claim that representation models in general are constructed by abstraction and concretisation. Consequently I am proposing a theory of theory-application.

6.2 The Structure of the Unified Model Hamiltonian

The unified model is the offspring of a hypothesis that combines all previous physical intuitions on nuclear structure. The goal is to overcome all shortcomings faced by earlier models. On the one hand, we know that strong interaction models are extremely schematic, because despite the large-strength nuclear forces the nucleons are expected to demonstrate some form of independent motion. In addition, strong interaction models are semi-classical and consequently not theoretically acceptable. On the other hand, (on the part of the independent particle models) the basic assumption of the shell models is that the nuclear potential is spherically symmetric. There is enough experimental evidence, however, to know that slow nuclear surface vibrations and static deformed nuclei occur, depending on the structure of the potential energy surface. Therefore, the single-particle orbits should depend on the form of the nuclear surface, that is on the spatial distribution of all nucleons. *Inter alia* this thinking led to the conclusion that there could be an interaction between collective and single-particle degrees of freedom, in short, to the unified model. Both of these kinds of modes of nuclear motion are combined linearly in the unified model Hamiltonian, which also involves an interaction term for the two modes:

$$H_{TOT} = H_{SP} + H_{COL} + H_{INT} \quad (6.1)$$

The first Hamiltonian term is that of the single-particle modes of motion. The general formulation of the unified model does not require a particular specification for this term. For the sake of keeping the discussion simple and for keeping along the lines of the exposition in the preceding chapter, let me assume shell structure with spin-orbit coupling by letting,

$$H_{SP} = \sum_i T_i + V(\mathbf{r}_i, \mathbf{l}_i, \mathbf{s}_i) \quad (6.2)$$

The second Hamiltonian term (in equation (6.1)) is that of the collective modes of motion. In fact, it was briefly mentioned earlier that empirical knowledge leads to the

conclusion that there are four kinds of nuclear collective behaviour; hence each kind must be manifested one way or another in this Hamiltonian term.

$$H_{COL} = H_{ROT} + H_{VIB} + H_{ROT-VIB} + H_{GR} \quad (6.3)$$

Nuclear motion due to fission is not an explicit part of the above Hamiltonian, but the phenomenon derives its explanation indirectly by reference to the second and third terms above (the vibration term and the rotation-vibration mixture term). The last term is the Hamiltonian term of giant resonance that is given explicitly below and will be discussed in some detail shortly. Each of the above terms could be given the following explicit form, which are easily reducible to the form used by the Copenhagen school (Bohr 1952, Bohr and Mottelson 1953) for the collective model.

$$H_{ROT} = \frac{(I-j)^2 \hbar^2 - (I_3 - j_3)^2 \hbar^2}{2\mathfrak{I}_0} + \frac{(I_3 - j_3)^2 \hbar^2}{16B\eta^2} \quad (6.4)$$

$$H_{VIB} = -\frac{\hbar^2}{2B} \left[\frac{\partial^2}{\partial \xi^2} + \frac{1}{2} \frac{\partial^2}{\partial \eta^2} \right] + \frac{1}{2} C_0 \xi^2 + C_2 \eta^2 - \frac{\hbar^2}{16B\eta^2} \quad (6.5)$$

$$H_{ROT-VIB} = \frac{(I-j)^2 \hbar^2 - (I_3 - j_3)^2 \hbar^2}{2\mathfrak{I}_0} \left[2 \frac{\eta^2}{\beta_0^2} - 2 \frac{\xi}{\beta_0} + 3 \frac{\xi^2}{\beta_0^2} \right] + \frac{(I_+ - j_+)^2 \hbar^2 + (I_- - j_-)^2 \hbar^2}{4\mathfrak{I}_0} \left[2\sqrt{6} \frac{\xi\eta}{\beta_0^2} - \frac{2}{3} \sqrt{6} \frac{\eta}{\beta_0} \right] \quad (6.6)$$

$$H_{GR} = \sum_{n,l,m} \hbar \omega_l^{(n)} (q_{lm}^{(n)} q_{lm}^{(n)} + \frac{1}{2}) \quad (6.7)$$

The parameters in the equations are defined as follows. The I and j are the total angular momentum of the system and the total angular momentum of the outer nucleons respectively, the components of which obey the standard commutation rules. The ξ , η , and β_0 correspond to the generalised coordinates and their derivatives, and \mathfrak{I}_i (for $i=1, 2, 3$) could be called the moments of inertia that are given above as functions of \mathfrak{I}_0 . The q_{lm} in the giant resonance Hamiltonian term are creation and annihilation operators for giant multipole resonance phonons.¹¹⁹

The third Hamiltonian term is that of the interaction of the single-particle and collective modes. The interaction of the motions must obey certain theoretical

¹¹⁹ For a more elaborate mathematical presentation and explication of all the terms of the collective Hamiltonian and the collective coordinates see Eisenberg and Greiner (1970).

restrictions. If we assume conservation of angular momentum and parity, then this term must be rotationally invariant and parity conserving. Furthermore, since we require time reversal invariance of the Hamiltonian, the kinetic energy must contain only products of an even number, i.e. even λ , of conjugate momenta. This term can thus be written in the following explicit form:

$$H_{INT} = \sum_i f(r_i) \sum_{\lambda\mu} \alpha_{\lambda\mu}^* Y_{\lambda\mu}^{(i)} \approx \sum_i f(r_i) \sum_{\mu} \alpha_{2\mu}^* Y_{2\mu}^{(i)} \quad (6.8)$$

If we exclude higher multipole deformations in $\alpha_{\lambda\mu}$ (e.g. octupole, hexadecupole etc.) we may write the approximate form of the Hamiltonian in terms of the quadrupole deformations, as given in the right-hand side of the above equation. The term $f(r)$ gives the strength and radial dependence of this interaction, generally given by $f(r) = -r\dot{V}_0(\alpha_{\lambda\mu} = 0, \mathbf{r}, \mathbf{l}, \mathbf{s})$ for a spherical shell model potential $V_0(\mathbf{r}, \mathbf{l}, \mathbf{s})$, under the assumption that the nucleon motion is very rapid compared to the collective motion.

If we were to focus our attention only on the total Hamiltonian structure of equation (6.1), then it is conceivable that we perceive the model as an extension of the single particle shell model. In other words, we could consider the second and third terms of the Hamiltonian as correction factors to the idealised single particle shell model, along the lines of McMullin's cumulative account of de-idealisation. We could possibly even go as far as to claim that the correction factors are introduced on purely pragmatic grounds. It is also conceivable that we could invent numerous hypotheses that give rise to the same structure, each one giving the model a different physical character, of course. But the physics of the model are dictated by just one underlying hypothesis, without which the model cannot be understood. The physical ideas behind the model are tacit components of the defining hypothesis and not explicit features of the model. These physical ideas are not the result of systematic theoretical considerations. They are the development of our physical intuitions about the nuclear domain, moulded by the successes and failures of earlier models. In view of the fact that they are not validated systematically by theory, they are *ad hoc* considerations to be tried out and ultimately judged by their success. This is why considerations about the defining hypothesis are important.

The hypothesis that underlies this model and gives rise to its structure is this: *Nucleons move nearly independently in a common slowly changing non-spherical potential*. This hypothesis is the starting point for understanding what the mathematical terms of the model Hamiltonian actually are. We notice that both the shell model hypothesis and the Fermi gas hypothesis are specific manifestations of the first clause in the above hypothesis, ‘nucleons move nearly independently’. In fact, any conceivable hypothesis that allows the nucleons to have single-particle degrees of freedom could be substituted for this clause of the unified model hypothesis. The fact that we impose shell structure to the unified model hypothesis is based on other additional hypotheses, which are motivated by independent reasons. In particular, by the fact that shell structure has been so far successful in accounting for single-particle degrees of freedom (the spin-orbit interaction hypothesis played an operative role in bringing about this success). Hence the justification for adopting a single particle shell model with spin-orbit coupling potential comes from reasons independent of the principles of quantum mechanics. This is so because it is after the success of the shell model that we have augmented the set of quantum mechanical bridge principles for the specific domain of nuclear structure. The two additional stock models associated with the newly established bridge principles are the Woods-Saxon potential and the Mayer spin-orbit coupling term.

The second component of the above hypothesis, ‘common slowly changing non-spherical potential’, is meant as a constraint on the independent motions of the nucleons. This is a departure from the collective model, which uses exactly the same Hamiltonian as the collective term here, but considers that to be the motion of the nucleus. In the unified model the nucleus as a whole exerts a collective potential that is non-spherical and affects, or restrains, the motion of individual nucleons. This clause, therefore, contains three implicit sub-clauses: (1) that there is a collective mode of nuclear motion which constrains the motion of individual nucleons, (2) that if the nuclear potential is to constrain the motions of individual nucleons then there must be an interaction between the single-particle modes with the collective modes of motion, (3) that if we assume the nuclear potential to change sufficiently slowly then we can make a physical approximation that may sanction the separation of the nuclear motion into single-particle and collective motions.

The character of the collective mode of nuclear motion, which constrains the motion of individual nucleons, must be investigated empirically since the theory does not provide us with this information. As already mentioned our experimental knowledge requires that we provide explanations for four kinds of nuclear collective behaviour, hence each kind must be manifested, one way or another, in this Hamiltonian term. The way the collective terms in equations (6.4), (6.5), (6.6), (6.7), are justified are along the same lines as the liquid drop model, analysed in section 5.2. The Classical form of the Hamiltonian is established by the use of a classical description of the nuclear collective motion. The modes of motion of rotation, vibration and the interactive mode of rotation-vibration are described in terms of classical parameters and classical collective coordinates, along the hydrodynamic analogy, and then converted into their quantum mechanical analogues. It was noted that quantum mechanics does not provide us with rules for this conversion which remains theoretically unjustified and arbitrary, in the sense discussed in the previous chapter. The same argument holds for the relevant terms of the unified model Hamiltonian. I shall not repeat the process in detail here, but I shall try to briefly explicate the construction of the giant resonance Hamiltonian term, and in doing so I shall have to reiterate the argument in schematic style.

Giant resonance motion is a phenomenon first detected in 1947. It involves density fluctuations in the nucleus that may be caused by the electric field of a coincident photon (γ -ray). The electric field of the photon acts only on the charged nucleons (i.e. protons) and because the nuclear centre of mass has to be at rest, the neutrons move in the opposite direction to that of the protons. Nuclei demonstrate various types of density fluctuations. For instance a dipole fluctuation (in an assumed spherical nucleus) involves the motion of the protons in roughly one hemisphere and of the neutrons in the opposite hemisphere. A quadrupole density fluctuation involves the motion of protons in two opposite quadrants of the sphere and of neutrons in the remaining two quadrants, and so forth for higher order fluctuations. This description of the giant resonance phenomenon is unquestionably classical. The concept of 'fluctuation density' is a classical concept, which conceals in it the analogy that the protons and neutrons behave like two classical fluids. In summary form let me sketch

the process of constructing the giant resonance Hamiltonian. We begin by using the hydrodynamic analogy for the fluctuation density (i.e. satisfaction of the Helmholtz equation and the homogeneous Neumann boundary condition) to find a general solution of the fluctuation density in terms of its collective coordinates. The most general solution to the classical time-dependent problem is an equation for the fluctuation density analogous to the defining equation (5.4) of the surface variables $\alpha_{\lambda\mu}$. From this equation, and continuing on the hydrodynamic analogy of a two-fluid system we determine an expression for the total energy of the system in terms of the collective coordinates. We then quantize the energy equation by introducing canonically conjugate momenta to the coordinates. And finally we define creation and annihilation operators for the giant multipole resonance phonons on the coordinates and momenta, to get the form of the Hamiltonian term in equation (6.7).

The point of this is to stress that the entire collective term of the unified model Hamiltonian is arrived at by quantization of the respective classical equations. The description of the collective motion is in terms of classical concepts, the modelling of all four terms of the system (i.e. equation (6.3)) is done by means of classical models, which are then quantized in customary, but arbitrary, ways. I have earlier used this argument, for the case of the liquid drop model, to argue that when we construct a representation model by using a classical description and then assign quantum mechanical properties to the constituent parts of the model, we do this because no stock model of quantum mechanics fits the description of the system in question. In such cases, when we lack the systematic use of the theory in the model construction, the construction of the model involves all the relevant background knowledge and the process of ‘theory entry’ involves the use of rules that are not provided by the theory. It follows that, when theory and model relate in such fuzzy ways, the only criterion we have for the model’s acceptance is its explanatory and predictive success. This is what it means for the model to have partial autonomy from the theory. This argument could be used to claim the partial independence of the unified model from quantum mechanics. However, the case of the unified model presents us with an additional interesting question: Why is it that we break up our *description* of the physical system into a quantum mechanical part (i.e. the single-particle motion) and a semi-classical part (i.e. the collective motion)? Addressing the process of construction of such a

sophisticated representation model (as the unified model) might shed some light on this question. In section 6.3, I shall attempt to give a view of the relation between theory and representation models that accommodates such features.

The second implicit element of the aforementioned clause is that, if the nuclear potential is to constrain the motions of individual nucleons then there must be an interaction between the single-particle modes with the collective modes of motion. This interaction is explicitly accounted for by the third term of equation (6.1) and implicitly by various components in the collective term of the Hamiltonian. The interaction term of the Hamiltonian is, in other words, chosen so that the assumption of non-sphericity is introduced into the total Hamiltonian, because a correction factor to the single particle shell potential that can bring about such an effect is not feasible. This is a departure from the shell model. The non-sphericity of the potential is introduced into shell structure as a consequence of the fact that single-particle and collective mode *coupling* is assumed. The single-particle Hamiltonian term, which represents shell structure, expresses only the individual nucleon motion. It does so, in an idealised way, i.e. by retaining the assumption of spherical symmetry within it. But spherical symmetry is not a part of the assumptions underlying the total unified model Hamiltonian. Often physicists talk of the *deformed* single particle shell model, but I take it that this is just convenient phraseology. What information is, in fact, contained in the total Hamiltonian becomes clear in a closer study of its terms. I shall simply give a caricature of this.

If the interaction between single-particle and collective motions is weak, we talk of *weak coupling*. Such is the case for spherical nuclei, whose physics are represented in terms of quadrupole vibrators. That is, the collective term of the total Hamiltonian reduces to that of an oscillator, similar to the liquid drop equation (5.10), because the potential is spherically symmetric hence rotary motion can be totally ignored. In addition, only quadrupole deformations need be considered in the collective and interaction terms of the Hamiltonian, since as we may recall quadrupole moments are used to measure the deviation of the nuclear density from spherical symmetry. Thus for weak coupling the Hamiltonian in equation (6.1) reduces to that of a quadrupole vibrator. If this were a good representation model for all nuclei then we could speak

of the collective and interaction terms as added correction factors to the single-particle term of the Hamiltonian, because collective and single-particle motions in this model are introduced as if they function almost independently of each other. However, this is only a very special case of the unified model generalised Hamiltonian given in equation (6.1). This Hamiltonian attempts to give the general picture of the deformed nucleus, to which the spherical nucleus is a limiting case. Hence in the more general case it is assumed that the interaction between the collective and single-particle motions is strong, and we talk of *strong coupling*.

When we look into the strong coupling case, i.e. the Hamiltonian given in the equations (6.1) to (6.8) above, we discern a number of features that although separated in the different Hamiltonian terms belong to the defining hypothesis of the total Hamiltonian. These features, which tell us how the single-particle motion is associated with the non-sphericity assumption, I shall label as follows: (1) the rotation-particle coupling (that corresponds classically to the Coriolis force), (2) the vibration-particle coupling and (3) the rotation-vibration-particle coupling. To discern this information we must regroup our total Hamiltonian of equation (6.1) into a more customary form:

$$H_{TOT} = H_{0sp} + H_{0col} + H' \quad (6.9)$$

Where, the first term corresponds to what we could call the single particle motion in a deformed axially symmetric potential. The second term corresponds to the rotation and vibration modes together with a modified rotation-vibration term. These two terms can now be seen as the results of two separate hypotheses whose mathematical representations are added linearly. The first hypothesis gives the information of a single nucleon undergoing motion under the influence of a spherically deformed potential. And the second hypothesis gives the information of a deformed nucleus undergoing motion together with an additional nucleon.¹²⁰ The third term is a compilation of various small terms that perturb the system described by the linear synthesis of the above two hypotheses. We have actually regrouped the Hamiltonian

¹²⁰ We could visualise this in analogy to the following classical case. The first part treats the motion of planet *A* in a potential field due to the rest of the solar system and the second part treats the motion of the entire solar system (together with planet *A*) relative to a fixed coordinate system.

in the standard form of two idealised linearly independent terms to which we add a perturbation term (as a set of correction factors). In most standard textbooks on nuclear physics, which do not emphasise the process of construction of the model, the unified model Hamiltonian is presented in this form. Below I include the explicit forms of the total Hamiltonian terms of equation (6.9) and for simplicity, I ignore the giant resonance term and include only the spherical harmonics associated with the quadrupole deformations:

$$H_{0sp} = \sum_i T_i + V(\mathbf{r}_i, \mathbf{l}_i, \mathbf{s}_i) + \beta_0 Y_{20} \sum_i f(r_i) \quad (6.10)$$

$$H_{0col} = \frac{\hbar^2}{2\mathfrak{I}_0} [I^2 - (I_3 - j_3)^2] + \frac{\hbar^2}{16B\eta^2} [(I_3 - j_3)^2 - 1] - \frac{\hbar^2}{2B} \left[\frac{\partial^2}{\partial \xi^2} + \frac{1}{2} \frac{\partial^2}{\partial \eta^2} \right] + \frac{1}{2} C_0 \xi^2 + C_2 \eta^2 \quad (6.11)$$

$$H' = \frac{\hbar^2}{2\mathfrak{I}_0} \mathbf{j}^2 - \frac{\hbar^2}{2\mathfrak{I}_0} [I_+ j_- + I_- j_+ + 2I_3 j_3] + \frac{\hbar^2}{2\mathfrak{I}_0} [I^2 - I_3^2 + \mathbf{j}^2 - j_3^2 - I_+ j_- - I_- j_+] \left[\frac{2\eta^2}{\beta_0^2} - 2 \frac{\xi}{\beta_0} + \frac{3\xi^2}{\beta_0^2} \right] + \frac{\hbar^2}{4\mathfrak{I}_0} [I_+^2 + I_-^2 + j_+^2 + j_-^2 - 2(I_+ j_+ + I_- j_-)] \left[2\sqrt{6} \frac{\xi\eta}{\beta_0^2} - \frac{2}{3} \sqrt{6} \frac{\eta}{\beta_0} \right] - [\xi Y_{20} + \eta(Y_{22} + Y_{2-2})] \sum_i f(r_i) \quad (6.12)$$

In this regrouping of the terms, it is easier to discern the various elements of the unified model defining hypothesis. For example, the non-sphericity of the potential is seen as -a correction- part of the single-particle Hamiltonian term. Yet, it is clear that it originates as part of the interaction term of equation (6.8). In addition, the terms of the perturbation Hamiltonian H' may now be recognised as follows: the first term is a pure single-particle correction term; the second is the rotation-particle coupling; the third and fourth terms describe the rotation-vibration interaction, the vibration-particle coupling and the rotation-vibration-particle coupling; finally the last term gives the influence of the individual nucleon on the surface vibrations. If the total unified model Hamiltonian is looked at in its regrouped form of equation (6.9), then it is reasonable to infer that all the above interactions are correction factors to an idealised initial hypothesis. The regrouping of the Hamiltonian makes the unified model look almost like the hydrogen atom case. To build a representation model for

the hydrogen atom, we start from a highly idealised theoretical model of quantum mechanics, that ironically we call the *hydrogen atom model*, and add a perturbation term (i.e. correction factors) to establish a more ‘realistic’ model of the actual hydrogen atom physical system. My claim, however, is that in the case of the unified model this position overlooks the process of construction. The construction begins with equation (6.1), which is a manifestation of a highly complex hypothesis that portrays our picture of the nucleus as it has evolved through the successes of our previous representation models. The fact that the terms of the total Hamiltonian can be regrouped in familiar ways of taxonomy only shows that we can take almost any Hamiltonian and express it in terms of a known term and some perturbation term. Although this is very useful for computational and for taxonomy purposes, it has very little to do with the actual process of construction. The fact that it is done indicates that the components of the model-defining hypothesis can be broken down and ordered according to their physical interpretation, and in ways that facilitate our computations. The *conceptual character* of the model-defining hypothesis however, which will be examined in Section 6.3, can be accurately apprehended by examining the actual process of construction.

Undoubtedly, the unified model is essentially a hybrid of shell model and collective model (liquid drop model). My question is, ‘what sort of hybrid is it?’ In regards to the physical conditions the unified model is closer to the shell model, that is, the nucleons move approximately independently rather than being strongly coupled. But the crucial connection between the unified model and the shell model is of an empirical nature, suggested by the fact that while collective motions in nuclei, to some extent, involve all the nucleons, the most loosely bound ones have proportionately the most effect. This connection is suggestive of how the unified model is constructed. By assuming the nuclear potential to change sufficiently slowly (which is the third implicit element of the aforementioned clause), we are making a physical approximation that sanctions the separation of the nuclear motion into intrinsic (i.e. single-particle) and collective motion. The first of these represents the motions of the nucleons in a fixed potential while the second is associated with variations in the shape and orientation of the nuclear field. This separation is in many respects analogous to the separation into electronic and nuclear motion in molecules.

What motivates this physical approximation? I think the answer to this (in addition to the obvious reason of being motivated by existing empirical knowledge) is primarily due to the surviving success of the shell and collective models. Once the intrinsic and collective modes of motion are separated we know we can represent the separate Hamiltonian terms by borrowing from the two predecessor models. So the unified model is a hybrid only by borrowing the mathematical representation of its separate terms, not by virtue of being an extension of the shell model. We can now see what sort of correction the interaction term in the Hamiltonian (of equation (6.1)) is. It is meant as an improvement for the approximation assumption that the nuclear potential changes slowly.

I believe that this understanding could only come by looking at the underlying hypothesis of the unified model. All the physical assumptions involved are contained in the hypothesis, not in the defined structure. If we focus solely on the structure of the model then any story would do. We could, for instance, look at the unified model and see an extension of the shell model that includes several correcting factors, which account for the collective modes of motion. In Chapter 3 I tried to explicate the fact that according to the SV, we define the intended models of the theory directly without recourse to formal syntax, by using the laws of the theory. But representation models do not belong to the class of intended models of the theory. They have ‘a life of their own’: their function is to extend the physical domain of application of the theory. Thus if we do not try to interpret them with reference to their underlying hypotheses we run the risk of making an epistemic mistake by reducing their physics down to the basic principles of the theory. This would practically be equivalent to discarding their physics altogether.

I have given an analysis of the hypotheses that underlie the unified model. Through such analyses, I argue, we can discern the evolutionary history of representation models. Since a model viewed as a mathematical structure cannot itself exhibit its own evolutionary history, I believe to have indicated a feature of model construction that is obscured by the SV. The above story enables us to discern one important element of our scientific inquiry into the world; that the progress in the construction of representation models in a particular physical domain relies, to a large extent, on

the explanatory and predictive successes of predecessor models. This use of representation models indicates how physical knowledge about a particular physical domain is accumulated, which, in its turn, shapes the defining hypotheses of successor models. It is also quite evident in the unified model case that the same arguments I gave for the previous models also hold. The model postulates mechanisms, initially in *ad hoc* ways, that later (if successful) come to be regarded as established bridge principles for the special case of the nuclear domain. Together with the spin-orbit hypothesis, in the case of the unified model we can also argue that the giant resonance hypothesis is established after postulating an *ad hoc* mechanism. These, *inter alia*, give the unified model its partial independence from theory and its explanatory power. Moreover, it is clear that the unified model is not developed by the use of the theoretical models of quantum mechanics, but by a complex variety of methods, such as building some of its pieces with classical models in mind and then furnishing them with quantum mechanical properties. I do not think it is necessary to rehearse these arguments. Instead, I would like to focus on another dimension of the above exposition of the unified model construction that may shed some light on a more accurate account of idealisation as practised in scientific theory application.

6.3 Further Remarks on Idealisation: The Process of Abstraction and Concretisation

Throughout this chapter I have advanced the view that our conception of the nature of a physical domain is shaped by the successes and failures of our respective representation models. So it comes as no surprise that the unified model is a hybrid of two predecessor representation models of the nuclear structure. I have argued that the unified model is not an extension of the single particle shell model, but an offspring of a defining hypothesis that makes use of the physics of the latter. The description I have given of the unified model indicates that to construct the model we start from a highly complex hypothesis about the nature of the nucleus. This hypothesis expresses our conception of the nuclear structure. The nucleus is a complex system of a collection of particles, which move about independently but at the same time

demonstrate a mode of motion as a collection, and that the two modes of motion exert influences on each other. It is upon this conception that our application of quantum mechanics in the nuclear domain rests. Evidently, different conceptions would lead to different representation models, i.e. different applications of the theory. This is the situation for the previously described cases of the liquid drop and the single particle shell models. We therefore need an account about theory-application that does justice to the variety of ways representation models are constructed, and also for the hypotheses that give rise to these models and manifest our conceptions of the physical systems. We have seen how McMullin's idealisation/de-idealisation cumulative account fails to capture the way by which nuclear models are used to explore the nuclear domain. In this section I want to develop a more generalised idealisation and de-idealisation account that I will call, following Suppe (1989) and Cartwright (1989), the processes of *abstraction* and *concretisation*. I shall briefly digress in order to clarify the need for such a terminological change, before attempting to elaborate this account and to explore its use in theory-application.

6.3.1 A Terminological Change: Abstraction and Concretisation

The term 'idealisation' is commonly used to label the thought process involved in the scientific methods traced historically to the Galilean methods. The term has been retained in more recent discussions that focus on models in contemporary science or more generally on applications of theories,¹²¹ and it is used to refer to concepts, systems, circumstances, conditions etc., which are either highly distorted descriptions of the real world, or descriptions from which many possibly relevant influencing factors have been neglected. Each for their own reasons, Cartwright and Suppe have both chosen to alter the terminology and speak of *abstractions* in science, instead of idealisations.

¹²¹ In addition to McMullin, some examples of such authors are Giere 1988, Shapere 1984, Laymon 1985 and 1995, Morrison 1997, 1998, and forthcoming (a).

It seems that for Cartwright the motivation for this terminological change is twofold. Firstly, the notion of idealisation disguises the idea of *changing* the particular features or properties of a physical system, whereas the notion of abstraction (which she traces back to Aristotle) is about *subtracting* features from the concrete circumstances. The second line of her motivation stems from her broader epistemological concerns. She argues that because idealisation is conceptualised as a change of the features of a concrete physical system, it follows almost naturally to speak of a ‘degree of departure from truth’ and consequently of a ‘notion of approximate truth’. In the case of the notion of abstraction, however, such expressions do not make sense, for we talk about a genuine subtraction of relevant features.¹²² Suppe’s motivation for suggesting a terminological change relies primarily on his observation that in our theoretical descriptions of the phenomena there are two kinds of thought processes involved. The first is the case of pure abstraction, i.e. relevant factors of the concrete system are genuinely subtracted from our theory or model. And the second is that of pure idealisation where certain features of the concrete system are distorted in our theoretical description. In the case of pure abstractions, it is ‘causally possible’ for the concrete system to realise the conditions dictated by our theoretical description, e.g. it is causally possible to isolate in the laboratory a concrete pendulum from air resistance that has been abstracted from the theoretical description. In the case of pure idealisations, however, the situation is such that it is causally impossible for the concrete system to realise the distortions imposed on the theoretical description. For example, it is causally impossible to bring the pendulum bob to realise conditions that are equivalent to having no extension.

I find both Cartwright’s and Suppe’s reasoning persuasive as to conclude that the notion of abstraction is much more general than the notion of idealisation and as such captures a broader spectrum of thought processes involved in scientific activity. Abstractions, as genuine subtractions of influencing factors, may also imply changes in the features of a concrete system. However, changes of the features do not necessarily imply the subtraction of factors, although the term idealisation as used by

¹²² As I do not focus on general epistemological issues in this work, for my purposes I am inclined to ignore the latter motivation altogether.

many authors is meant to designate Aristotelian abstraction too. Abstractions as genuine subtractions may also imply distortions of the features of the concrete system. However, distortions (i.e. idealisations) are only a very special kind of subtraction of features. These thoughts lead to the conclusion that idealisation, as understood in the present context, is a special form of abstraction, and not the converse.

But I believe there is an additional reason why the notion of abstraction can be more apt and useful than idealisation. It allows us to draw a distinction of how the same concept is used inter-theoretically (e.g. in classical mechanics and in the special theory of relativity) and intra-theoretically (e.g. in a kinematical sense and in a dynamical sense). As an example I will elaborate on the inter-theoretic use of the concept of a 'rigid body'. A rigid body is defined in classical mechanics as a body for which the distance between any two of its parts remains invariant under rotation, vibration and rectilinear motion. Although no such bodies are found in nature, what is interesting is that classical mechanics does not impel us to the commitment that rigid bodies are a physical impossibility. The special theory of relativity also employs (for reasons that I will not attend to) the classical concept of a rigid body.¹²³ But we must be cautious of a significant difference in the usage. In the case of the special theory of relativity, we are supplied with scientific grounds by which to consider the notion of a rigid body referring to a 'fictitious' entity, i.e. a physical impossibility. Given the above classical definition of the concept, if a force is applied to a body, in order for it to remain rigid, the force must be transmitted instantaneously to all its parts. For this to happen, the force must be transmitted with infinite velocity. The special theory of relativity, as is well known, does not allow the transmission of energy and momentum, and consequently forces, with velocity greater than that of the velocity of light in vacuum. Hence, in order to avoid the apparent contradiction we must settle with holding the classical concept of a rigid body as a fictitious entity, although the concept is useful in the exposition of the theory whose fundamental principle it contradicts.

¹²³ Shapere (1984) gives a very interesting analysis of how the concept of a rigid body is employed in the special theory of relativity, and contrasts it to its use in rigid body mechanics.

In the terminology of idealisations we are forced to call both uses of the ‘rigid body’ concept idealisations. Yet the uses of the concept in the two theories are distinctly different. We need to clarify how they differ or else change our terminology to one that contains the clarification within it. I think the terminology of ‘abstractions’ does contain clarifications of this kind within it. To arrive at the definition of a rigid body, in the classical usage of the concept we abstract features and properties of concrete bodies, thus distorting the phenomena. That is, our abstraction is of the special case of a pure idealisation but only by virtue of the fact that we have not found such bodies in nature, not because classical mechanics prohibits their existence. In the relativistic usage of the classical notion of ‘rigid body’ however, we employ an approximate definition of a relativistic analogue concept. The classical sense of ‘rigid body’ is according to the special theory of relativity not an idealisation, but referring to a fictitious entity. It is used only for the convenient exposition of the theory. In fact we could instead employ a relativistically consistent definition of a rigid body, such as, being a body in which the maximum velocity that impulses could be transmitted is the speed of light. This relativistic sense of a rigid body is now an abstraction within the special theory of relativity in the same way as the classical notion is an abstraction within classical mechanics. It is, what Suppe would have called, a pure idealisation by virtue of the fact that we have not observed such bodies (and possibly they do not exist) with a dielectric constant equal to 1, and not because the theory prohibits their existence. So, it may be said, both classical and relativistic ‘rigid body’ concepts are idealisations. But this is only the case relative to the theory in which they are used. Without such qualification the terminology of ‘idealisations’ is inaccurate. Because, according to the special theory of relativity the relativistic notion is an idealisation and the classical notion is scientifically fictional. We must somehow distinguish the two without reference to the theory in which they are used, and the terminology of idealisations does not help. With the terminology of abstractions this inter-theoretic distinction comes naturally. The relativistic concept is formed by several abstractions but the classical notion involves an additional abstraction of a property that happens to be in violation of a fundamental principle of the special theory of relativity: namely, the theoretical feature of the maximum limit to the velocity of transmission of impulses is subtracted.

Having expressed my rationale for changing the terminology from the ‘process of idealisation’ to that of the ‘process of abstraction’, I also want to give my reasoning for changing the terminology for the converse process of de-idealisation. De-idealisation is the term commonly given to the process of bringing a theoretical description closer to the occurrences in an actual physical system. In sub-section 5.3.2 I analysed McMullin’s de-idealisation account as a view of how theories are applied to the phenomena. I emphasised the fact that it involves the cumulative introduction of correction factors to our initial model about a physical system. The key features of this account are two, that the introduction is cumulative and that the correction factors are dictated by the conceptual resources of the scientific theory that gives rise to the initial model. In sub-section 5.3.2 I argued against the second feature, by claiming that the spin-orbit hypothesis is not dictated by any theory. Shortly I will try to argue against the generality of the first feature of this account. To do this it is convenient, when we talk about this process, to think about it as incorporating features into the abstract description given by our model such as to turn it into a more concrete description. Incorporating more features to construct a more concrete version of our model may be done by the cumulative introduction of influencing factors, as is the case of the simple pendulum. However, I have already argued that for the case of the unified model we incorporate concrete features by synthesising them in the model-defining hypothesis. This synthesis cannot be viewed as a cumulative process, and since the terminology of de-idealisations has become customary for the cases where we make changes (understood as addenda to the Hamiltonian operator or the force function) to the model to bring it progressively closer to the truth, I prefer to drop the talk about de-idealisations altogether. As a substitute I shall use the term *concretisation* of the model, which I believe captures the synthesis of the various concrete ingredients convoluted in the model-defining hypothesis, without implying the commitment that the concrete features originate within the conceptual resources of the theory.

In addition, the limiting case of de-idealisation is traditionally understood as the introduction of all the factors that the theory suggests for the improvement of the representation capacity of our model. The term ‘concretisation’, however, should be

understood as referring to the representation model itself, not the theory. The limiting case of concretisation of representation models is, of course, the inclusion of all factors that are present and active in the physical system. Hence, both the inclusions of factors motivated by theoretical considerations, and those motivated by experimental considerations count as concretisations of the model. This terminology, therefore, abides to my view that representation models have a degree of autonomy from the theory that gives rise to them.

6.3.2 The Process of Abstraction and Concretisation

The process of abstraction in scientific theorising enters at different levels. I want to identify two principal levels of abstraction that are useful to our understanding of how theories are applied. Assuming that we begin with the universe of discourse, the first level of abstraction I want to distinguish is that of selecting a small number of variables and parameters abstracted from the phenomena and used to characterise the general laws of a theory. For example, in classical mechanics we select *position* and *momentum* and establish a relation amongst the two variables, which we call Newton's 2nd law or Hamilton's equations. We have no justifiable reasons to eliminate the possibility that the kinematics and dynamics of bodies are influenced by factors that are related, for example, to electrical or heat phenomena. By abstracting a set of parameters we thus create a sub-domain of the universe of discourse, which we call a scientific theory. Thus Newton's laws signify a conceptual object of study that we may call the domain of classical mechanics, as long as we distinguish it from having any direct reference to physical domains. Similarly Maxwell's equations signify the domain of classical electromagnetism, the Schrödinger equation signifies the domain of quantum theory, and so forth. In all these laws (which we may call abstract, in the sense that they are established by a small number of abstracted parameters) something is left unspecified: the force function in Newton's 2nd law, the electric and magnetic field vectors in Maxwell's equations, and the Hamiltonian operator in the Schrödinger equation. We understand that the specification of these is what would establish the link between the assertions of the theory and physical systems.

So far, my description may seem indistinguishable from Suppe's Semantic View version. The distinguishing elements lie in the process of specification of the force function or the Hamiltonian operator. Suppe maintains, along with all the proponents of the SV, that this specification is along the lines of a definition. For instance, we are confronted with a real pendulum that we want to model via Newton's 2nd law. We know that we can define the mathematical form of the force function to be proportional to the displacement of an oscillating body from its equilibrium position. This defined force function is associated with a model that resembles some of the features of our real pendulum. The resemblance of these features, Suppe claims, is grounded in what he calls the abstract and idealised replicating relation (that we may recall from chapter 3). The defined model is an abstract and idealised replica of the real pendulum. The question as to how we could increase the degree of resemblance of our model to the real pendulum is practically meaningless for his view. All we need to do is define a new force function that accounts for more features of the real pendulum and thus minimise the gap of the replicating relation between the model and the real pendulum. Suppe's theory of abstraction stops here because he makes the same assumption as van Fraassen, that the conceptual resources of the theory are all that is necessary in applying the theory and all that is used in constructing representation models.

I have been arguing all along that this is not enough. We need a view of theory-application that can accommodate our constructions of representation models. For this we need to expand our understanding of the process of abstraction to also explicate the process of specifying force functions and Hamiltonian operators. This is the second principal level at which abstraction enters in our theorising and in which I am mostly interested; it is effective in allowing us to relate the assertions of the theory to physical systems. Assume that we begin with a formulated theory, such as quantum mechanics, in which case the starting point is the Schrödinger equation: $i\hbar \frac{\partial \psi}{\partial t} = -H\psi$. When we are faced with the problem of applying the Schrödinger equation to a particular physical system, we are in fact facing the problem of specifying a Hamiltonian operator for the representation of the system. The

specification of the Hamiltonian operator relies on our conception of the particular physical system, which is expressed in a model-defining hypothesis. Our hypotheses can be such that we can specify Hamiltonian operators in two distinct ways. The first way is to specify a Hamiltonian straight out of the list of stock models of quantum mechanics. The second way, however, requires the use of background knowledge about the physical system from wherever we can gather it, and is subject to continuous development.

If we use one of the stock models of quantum mechanics to represent the system, in effect we make a hypothesis about the physical system that involves an assemblage of abstractions. Not many systems (if any) in the real world behave like potential wells or harmonic oscillators, or the like. Therefore, our hypothesis is asserted in a counterfactual way: given that these factors are abstracted from the physical system, the system will behave as a harmonic oscillator. In other words, the formulation of the hypothesis forecasts what factors should be later reintroduced to improve the representation capacity of the Hamiltonian operator. What is usually done in quantum mechanics, is to add a perturbation (correction) term to the initial Hamiltonian. Sometimes this perturbation term is dictated by systematic theoretical considerations, but this is not always the case. Other times we have to search for corrective terms by using our physical insight into the particular physical domain, and quantum mechanics does not always play a central role in this exploration. The single particle shell model is a case where our initial hypothesis allows the use of a stock model, and forecasts what abstractions (e.g. spherical symmetry) must be overcome in order to concretise the model and consequently improve its representation capacity. Quantum theory, however, does not suggest ways by which to deal with the spherical symmetry that the hypothesis imposes and neither does it suggest how to account for inter-nucleon interactions. These concretisations to the single particle shell model Hamiltonian have to come from theory-independent considerations. The spin-orbit coupling brings, at least partially, these desired effects.

Cartwright (1989, pp202-206) has addressed the processes of abstraction and concretisation as a theory-application view of the above kind and suggested the

following account.¹²⁴ Let T^k be the defining hypothesis of factor H , which in a realistic description R is functionally related to factors P_1, \dots, P_n and S_1, \dots, S_k . We could think of the P_i 's as the primary factors of influence on H and the S_i 's as the secondary factors of influence. The process begins with formulating an abstract hypothesis of the following form:

$$T^k: \text{ If } R(x) \text{ and } S_1(x) = 0, \dots, S_k(x) = 0, \\ \text{ then } H(x) = f(P_1(x), \dots, P_n(x))$$

The statement says that if we abstract all the secondary factors of influence from the realistic description, then H would be functionally related only to the primary factors of influence. The step by step process of concretisation of our hypothesis, that would improve the representation capacity of our model, involves the gradual addition of the secondary factors. A first step concretisation would be the following:

$$T^{k-1}: \text{ If } R(x) \text{ and } S_1(x) = 0, \dots, S_{k-1}(x) = 0, \text{ and } S_k(x) \neq 0, \\ \text{ then } H(x) = g_{k-1}[f(P_1(x), \dots, P_n(x)), h_k(S_k(x))]$$

The new relation g_{k-1} , between H and its influencing factors, is functional of the previous one f plus the function h_k , which expresses the impact of S_k on the magnitude of H . If we carry this process to the limiting case of final concretisation, where all the subtracted factors are introduced, we are left with a statement of the following form, in which the impact of all secondary factors on the magnitude of H is included in the final functional relation:

$$T^0: \text{ If } R(x) \text{ and } S_1(x) \neq 0, \dots, S_k(x) \neq 0, \text{ then} \\ H(x) = g_0[f(P_1(x), \dots, P_n(x)), h_k(S_k(x)), \dots, h_1(S_1(x))]$$

This concretisation account avoids the two problems faced by McMullin's de-idealisation account. Firstly, it does not imply that the correction factors are

¹²⁴ Cartwright attributes this account to Leszek Nowak. I reproduce the suggestion from Cartwright (1989, p204) in a slightly altered notation, more suitable for my purposes.

introduced in a cumulative manner, in fact they all exert some impact in the overall functional relation which implies that their impact could also come from their mutual interactions. Secondly, we may assume that these factors are causally influential on H but not necessarily described by the theory's conceptual apparatus. Yet despite the improvement over McMullin's view, this account is a simplification of the conceptual process of abstraction and concretisation and does not capture the more general form of theory-application we encounter in examples of the second kind. It shares with McMullin's view the weakness of being what I choose to call a 'one-dimensional' account of concretisation, or de-idealisation, of model construction.

The second way by which to specify a Hamiltonian and apply quantum theory, which the unified model exemplifies, is far more intricate. Through the successes of earlier models in accounting for different properties of the nucleus, our conception of the nuclear structure has become progressively more detailed. We now have a far more 'realistic' description of the nucleus, and we also know how to model it mathematically. As a consequence, the hypothesis that specifies the Hamiltonian, associated with the model, has a relatively concrete form. The unified model hypothesis asserts that the nucleus exhibits some form of independent nucleon motion, but that this motion is constrained by a slow collective motion of a core of nucleons, and that the two modes of motion interact with each other. In addition it asserts that the collective mode of motion is constituted by three distinct kinds of motion (vibration, rotation and giant resonance), two of which demonstrate an interaction mode. No doubt, a number of factors in our description of the nucleus are abstracted, for instance we still talk of a core of nucleons that do not demonstrate independent motion. In its entirety, this is however a relatively concrete hypothesis, and thus it does not fit the above logical schema suggested by Cartwright and Nowak.

The case of the unified model is one where the hypothesis asserted is not in its entirety in a highly abstract form. It involves many of the significant features of the nuclear structure that are present in our description of the physical system. Nevertheless, in specifying a Hamiltonian we abstract by dividing these features into three separate terms (recall equation (6.1)), as if their contribution to the behaviour of the nucleus is distinct and autonomous. This procedure is very frequent in modelling

in physics, but we must recognise that it is only a conceptual division. The three terms in the unified model Hamiltonian are not meant to act disjointedly nor to represent separately, we impel the division by abstracting. The abstraction involved is the foundation of the counterfactual assertion, implied by the Hamiltonian, that the overall nuclear motion is as if it receives contributions from distinct and autonomous influencing factors. This way by which abstraction is used in our modelling must be reflected in our logical schema of abstraction and concretisation. Thus making our logical schema more general ('multi-dimensional', as I suggest to call it) as to account for cases where the hypothesis is asserted in a rather concrete form.

After we have settled this point we can see where the process of concretisation becomes operative. Concretisation is involved in bringing every individual term of the Hamiltonian closer to reality, as if it functions alone. And also in bringing closer to reality the interacting terms, thus compensating for the assumption that the separate terms are disjoint and autonomous. So the process of concretisation in the unified model involves the addition of all the factors that are considered functional in the individual Hamiltonian terms of single-particle mode, collective mode, and interactive mode of motion. As schematised by Cartwright concretisation does not capture these functions. The logical schema I want to suggest, to capture this thought process, is an extended multi-dimensional version of Cartwright's and Nowak's account and is as follows:

$T^{\alpha\beta}$: If $R(x)$ and $S_{11}(x)=0, \dots, S_{ij}(x)=0, \dots, S_{\alpha\beta}(x)=0$, and if $P_{m1}(x), \dots, P_{mn}(x)$ act on the physical system autonomously from $P_{k1}(x), \dots, P_{kl}(x)$, then

$$H(x) = f_1(P_{11}(x), \dots, P_{1\gamma}(x)) + f_2(P_{21}(x), \dots, P_{2\xi}(x)) + \dots + f_\delta(P_{\delta 1}(x), \dots, P_{\delta \varepsilon}(x))$$

The statement $T^{\alpha\beta}$ says that in a realistic description R of a physical system we abstract in two distinct ways. Firstly we abstract by categorising the factors of influence into primary, P 's, and secondary, S 's, and by subtracting all the secondary factors of influence from our initial theoretical description. Secondly we abstract by grouping the primary factors into separate terms, f_i 's, each of which is assumed to act autonomously on the physical system, and by categorising the secondary factors into

their corresponding groups. Then H would be the sum of terms, each of which is functionally related only to different primary factors of influence. The step by step process of concretisation of our hypothesis, that would improve the representation capacity of our model, involves the gradual addition of the secondary factors related with each and every one of the individual Hamiltonian terms. A first step concretisation would be the following:

$$T^{\alpha\beta-1}: \text{ If } R(x) \text{ and } S_{11}(x) = 0, \dots, S_{\alpha\beta-1}(x) = 0, \text{ and } S_{\alpha\beta}(x) \neq 0, \text{ and if } \\ P_{m1}(x), \dots, P_{mn}(x) \text{ act on the physical system autonomously from } P_{k1}(x), \dots, P_{kl}(x), \\ \text{ then } H(x) = f_1(P_{11}(x), \dots, P_{1\gamma}(x)) + \dots + g_{\alpha\beta-1}[f_\alpha(P_{\alpha1}(x), \dots, P_{\alpha\eta}(x)), h_{\alpha\beta}(S_{\alpha\beta}(x))] \\ + \dots + f_\delta(P_{\delta1}(x), \dots, P_{\delta\epsilon}(x))$$

Where I have just added the influence of just one secondary factor ($S_{\alpha\beta}$) in just one of the Hamiltonian terms ($g_{\alpha\beta-1}$). This goes only to show that concretisation factors are added only to individual Hamiltonian terms, it does not portray the actual practice in science, where concretisation factors may be added simultaneously or after significant theoretical and experimental developments. It must be noted that this logical schema allows for the regrouping of the Hamiltonian terms, as well as for the introduction of new terms as correction factors or as addenda. In other words, it allows for radical improvements to representation models in a particular physical domain that usually come about after a breakthrough is accomplished. Hence, I suggest that we view the move from the single particle shell and collective models to the unified model in this manner. A final concretised assertion would have the following form:

$$T^{00}: \text{ If } R(x) \text{ and } S_{11}(x) \neq 0, \dots, S_{\alpha\beta}(x) \neq 0, \text{ and if } \\ P_{m1}(x), \dots, P_{mn}(x) \text{ act on the physical system autonomously from } P_{k1}(x), \dots, P_{kl}(x), \\ \text{ then } H(x) = g_{10}[f_1(P_{11}(x), \dots, P_{1\gamma}(x)), h_{11}(S_{11}(x)), \dots, h_{1\theta}(S_{1\theta}(x))] + \\ g_{20}[f_2(P_{21}(x), \dots, P_{2\psi}(x)), h_{21}(S_{21}(x)), \dots, h_{2\chi}(S_{2\chi}(x))] + \dots + \\ g_{\delta 0}[f_\delta(P_{\delta1}(x), \dots, P_{\delta\epsilon}(x)), h_{\delta1}(S_{\delta1}(x)), \dots, h_{\delta\varphi}(S_{\delta\varphi}(x))]$$

The final statement T^{00} says that in a theoretical description of a physical system, in which all known factors of influence that were initially abstracted from the realistic description R are now reintroduced, we have an expression that breaks down the impact of all influencing factors into several terms each of which is assumed to act autonomously on the physical system. I believe that this account captures well the construction process of the unified model Hamiltonian. It also points to how we break up our *description* of the physical system into a quantum mechanical part (i.e. the single-particle motion) and a semi-classical part (i.e. the collective motion), and come up with a Hamiltonian operator that combines these non-dictated by quantum theory features. Subsequently it sheds some light on how representation models relate to the theory. Moreover, it explicates one other important element of actual model construction. Each different term of the Hamiltonian carries its own separate, and frequently independent, assumptions. It is in my opinion a mistake to regard these as assumptions of the total Hamiltonian. Spherical symmetry, for example, is an abstraction of the single-particle term of the total unified model Hamiltonian. The constraint of sphericity is relaxed in the collective term, hence the abstraction in the single-particle term is a residue that ‘haunts’ our model. This is one reason why in section 5.3.2, I criticised the traditional view of de-idealisation because it is a partially ordered process. Indeed, concretisation, as I suggest it functions, involves leaps from one Hamiltonian term to the next, each correction belongs to the term in which it applies, it is too simplistic to view different Hamiltonian terms as corrections to another. Science is not carried out in an ideal world, hence ready-made recipes for constructing representation models are absent from the scientist’s cookbook. The only criterion for the success of the model is its explanatory and predictive power, and as philosophers we should reconstruct the scientific activity as such.¹²⁵

Finally, it is important that we distinguish between the procedure of abstraction and the decision of what factors are to be abstracted. The procedure of abstraction I

¹²⁵ Notice that the issue of approximation can be accommodated within this account by introducing approximate values for the influencing factors, thus yielding a concretised and approximate hypothesis. Of course, adjusting a theory of abstraction and concretisation with considerations on approximation, is an issue that must be the subject of more careful and detailed work.

suggest, is an explication and description of a thought process that takes place in model construction. It is thus a philosophical rational reconstruction, that is to say a simulacrum, of certain elements of scientific activity. The decision as to what factors are to be abstracted is part of scientific activity *per se*. Amongst other things, the latter involves theoretical competence, rich and sound experience in the application of a theory to a particular physical domain, and possibly complex inductive techniques. In making this distinction I, therefore, distinguish my suggested account from work on questions of *how* abstractions or idealisations are used in science.¹²⁶ Questions, that is, which draw feedback from idealised manipulations of theories in theory-application procedures. Instead, my questions are oriented around *what* abstractions (or idealisations, if you like) are and *what kinds* of abstractions are employed in the constructions of scientific theories and representation models, and finally how do we confront the question of *how* theories propounded as abstractions or idealisations may be related to phenomena. My motivation stems from understanding *scientific theories propounded as abstractions*, thus my philosophical worries are concerned with how we establish reasonable links of such theories with the more concrete tools of scientific inquiry, i.e. representation models, and subsequently relate them to the phenomena. In this quest, my starting point was Suppe and Cartwright, from whose work it is apparent that I draw. But for different reasons that I hope to have made clear, I find their work inadequate to capture the complexity of this relation.

6.4 Conclusion

I have tried to show how the unified model of nuclear structure is constructed. I hope to have demonstrated the partial autonomy from theory the model has. A feature which, following Morrison, I attribute to the explanatory and predictive success of the model itself, but also on the fact that its construction relies heavily on the explanatory and predictive success of the single particle shell model and the collective model. I have also tried to show that the construction of representation models, especially

¹²⁶ See in particular Giere 1988a, Shapere 1984, McMullin 1985, Laymon 1985, 1995, Morrison 1997, 1998, and forthcoming (a); these are of course authors from whose work I draw heavily.

those of high complexity as the unified model, are not a matter of definition as the proponents of the SV would have it. In fact if we look at them as defined structures, then we must recognise that they are subject to a defining hypothesis. The latter is a manifestation of our conception of the particular physical system or of the elements of a particular physical domain. As such, it is always subject to change as our knowledge of the domain improves; moreover, this improvement in knowledge goes hand in hand with the successes and failures of representation models.

Obviously the process of constructing a representation model is a complex activity, far more complex than what formalistic analyses of scientific theories, like the SV, seem to suggest. Surely it is not a process of having a theory completely laid out and just choosing the model of the theory we want to test. The models of the theory play a heuristic role in many different steps of the representation model construction, but so do other subsidiary hypotheses about the physical system. But, this is all the models of the theory do; the final construction, as Morrison puts it, has a life of its own. Its explanatory power is what we must recognise. The explanatory and predictive power of the unified model accounts for and corroborates the principles of quantum mechanics, but also for the subsidiary hypotheses about our conception of the physical system that played a vital role in the construction.

I have also tried to give a general philosophical account of the construction of representation models, what I have called the process of abstraction and concretisation. This account suggests a direction of how we should think of the relation between theory and representation model. If the account is a worthwhile explication of the process of model construction, then it could guide us to look for a better theory of representation than the ones suggested by the proponents of the SV. It also could direct us in how to improve on our theories of confirmation. But these are issues for future detailed work.

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